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DISCUSSION OF AERODYNAMIC THEORY OF BRIDGE OSCILLATIONS

(*Published in October, 1949*)

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DISCUSSION

DAVID J. PEERY,[25] M. ASCE.—The writer had the privilege of discussing and checking much of the author's work during the preparation of the paper, which was completed in essentially the present form early in 1947. The author has combined extensive research into the fields of aerodynamics and vibrations with his knowledge of suspension bridges. This paper presents a reliable and practical analysis. The following discussion will further confirm the validity of using tests on static models.

An oscillating suspension bridge is under the action of elastic, inertia, damping, and aerodynamic forces. The elastic and inertia forces may be computed accurately, and the natural modes of vibration resulting from these forces have been obtained by several investigators and verified by model tests. The structural damping forces are more difficult to determine, since models do not simulate the true conditions in the prototype. Fortunately it is conservative to assume a limiting case of no damping.

The aerodynamic forces on a thin airfoil oscillating in an ideal fluid have been obtained theoretically. Theodor von Kármán, Hon. M. ASCE, and W. R. Sears[26] have shown that these forces may be resolved into three components as follows:

(*a*) Forces that would be produced if the wake had no effect, called "quasi-steady" forces;
(*b*) Forces produced by the reaction of the fluid accelerated by the motion of a solid body, or "apparent mass" forces; and
(*c*) Forces that depend on the vorticity distribution in the wake.

The forces in group (*a*) are readily obtained by wind-tunnel tests of straight and curved models under steady flow conditions. The forces in group (*b*) are in phase with the inertia forces acting on the oscillating mass of the structure, and are negligible in comparison for suspension bridges. The forces in group (*c*) may be evaluated theoretically for a thin airfoil, but a similar analysis for a suspension bridge cross section appears impossible.

The theoretical thin airfoil, shown in Fig. 24(*a*), has a lift force of $\pi \rho V^2 b \alpha$ acting at the quarter chord point. Similarly, the curved thin airfoil in Fig. 24(*b*) has a lift force of $\pi \rho V^2 b \beta/2$ acting at the midpoint. These theoretical forces differ slightly from experimental values because the airflow tends to separate from the leading edge of a thin plate in violation of the assumed condition of a rounded leading edge. For the conditions shown in Fig. 8(*b*) the the-

NOTE.—This paper by D. B. Steinman was published in October, 1949, *Proceedings*. The numbering of footnotes, equations, and illustrations in this Separate is a continuation of the consecutive numbering used in the original paper.

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[26] "Airfoil Theory for Non-Uniform Motion," by Theodor von Kármán and W. R. Sears, *Journal of the Aeronautical Sciences*, August, 1938, p. 379.

oretical pressures are negative at all points, and are symmetrically distributed as the ordinates of a semi-ellipse. The positive experimental values shown result from flow separation at the corner of the leading edge, and this condition also exists for practical bridge roadways.

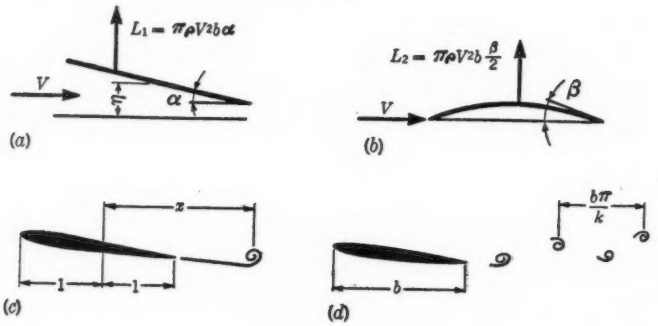

Fig. 24

For the oscillating thin airfoil, the effective angle of attack is decreased because of the vertical velocity $\dot{\eta}$. The vertical velocity varies across the width, and has a value of $\dot{\eta} - \frac{b\,\dot{\alpha}}{4}$ at the third quarter chord point, which determines the angle of attack of the airfoil. The effective angle of attack is therefore $\alpha - \frac{\dot{\eta}}{V} + \frac{b}{4}\frac{\dot{\alpha}}{V}$. Substituting this value for the lift force in Fig. 24(*a*) the following value is obtained:

$$L_1 = \pi\,\rho\,V^2\,b\left(\alpha - \frac{\dot{\eta}}{V} + \frac{b}{4}\frac{\dot{\alpha}}{V}\right) \ldots\ldots\ldots\ldots\ldots\ldots (41)$$

The effective angular velocity at the midchord point is equal to $\dot{\alpha} - \frac{\ddot{\eta}}{V}$. From the author's discussion of curved models, this is equal to $\frac{2\,V\,\beta}{b}$, with β as shown in Fig. 24(*b*). The following lift force resulting from the effective angular velocity is obtained from Fig. 24(*b*), thus:

$$L_2 = \frac{\pi}{4}\rho\,V\,b^2\left(\dot{\alpha} - \frac{\ddot{\eta}}{V}\right) \ldots\ldots\ldots\ldots\ldots\ldots (42)$$

The last term of Eq. 42 represents the "apparent mass" effect, and is equal tc the inertia force on a cylinder of air of diameter b. The other terms of L_1 and L_2 are quasi-steady forces, which the author obtains from wind-tunnel tests of static models similar to those shown in Figs. 24(*a*) and 24(*b*).

When an airfoil is suddenly moved so that the lift is changed, a vortex is shed from the trailing edge, and this vortex moves downstream with the fluid as shown in Fig. 24(*c*). The lift on the airfoil does not reach its final value until this vortex has moved an infinite distance downstream, and the lift is considerably smaller when the vortex is near the airfoil. The vortex trail behind an

oscillating airfoil is shown in Fig. 24(*d*), and the reduction in lift, $-L_3$, resulting from this vortex trail, may be expressed in the following form:

$$-L_3 = (1 - C)\, L_1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (43)$$

The term C is evaluated by Theodore Theodorsen[2] by considering the vortex trail between the trailing edge ($x = 1$) and infinity; thus:

$$C = \frac{\displaystyle\int_1^\infty \frac{x\, e^{-ikx}\, dx}{\sqrt{x^2 - 1}}}{\displaystyle\int_1^\infty \frac{(x + 1)\, e^{-ikx}\, dx}{\sqrt{x^2 - 1}}} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (44)$$

These integrals are evaluated as Bessel functions. Numerical values of $C = F + i\,G$ have been plotted for values of $k = \omega\, b/(2\, V)$ by Mr. Theodorsen, and have been reproduced by Friedrich Bleich,[27] M. ASCE.

The total lift force is obtained as the sum of the components, L_1, L_2, and L_3, as follows:

$$L = \pi\, \rho\, V^2\, b\, C \left(\alpha - \frac{\dot{\eta}}{V} + \frac{b\, \dot{\alpha}}{4\, V}\right) + \frac{\pi}{4}\, \rho\, V\, b^2 \left(\dot{\alpha} - \frac{\ddot{\eta}}{V}\right) \ldots\ldots\ldots (45)$$

Eq. 45, obtained by combining the steady flow conditions of Figs. 24(*a*) and 24(*b*) with the wake effects of Fig. 24(*d*), corresponds with the values obtained by Mr. Theodorsen and other investigators from considerations of the velocity potential for unsteady flow conditions. This verifies the author's procedure of using static models to obtain the quasi-steady forces.

The forces resulting from the wake vorticity will be considerably different for a bridge cross section than for an airfoil. Eqs. 43 and 44 are derived for an airfoil by assuming that the Kutta condition, in which the flow leaves the sharp trailing edge, is satisfied at any instant. For an oscillating bridge cross section with a blunt trailing edge, the trailing vortices may be shed at various points. The author has evaluated the wake effects from the pressure distribution on a static model, considering a phase lag. This procedure involves more approximations than were used in obtaining the quasi-steady forces, but appears to be the best method available for evaluating these effects.

At present, two methods are available for calculating aerodynamic forces on oscillating bridge sections. Mr. Bleich[27] uses the basic forces for a thin airfoil and superimposes an alternating force that must be determined empirically from dynamic model tests. This method applies only to open truss structures which approximate a flat plate. The author determines all forces from static model tests, and thus obtains a solution which is applicable to all cross sections, including girder-stiffened roadways.

In reviewing the present state of knowledge of suspension bridge vibration analysis, it appears that a designer who makes use of the available theory may be confident of the safety of his structure. Static models should be tested carefully. The models should span between walls of the tunnel to insure two-dimensional flow. Since flow separation effects are important, the Reynolds

[27] "Dynamic Instability of Truss-Stiffened Suspension Bridges Under Wind Action," by Friedrich Bleich, *Transactions*, ASCE, Vol. 114, 1949, p. 1177.

number for the model flow should correspond with that for the prototype. Tests on a dynamic model of the final structure are probably advisable in order to verify the calculations of aerodynamic forces.

Since aerodynamic forces cannot be obtained exactly, some conservatism in the design for wind forces is desirable. However, it seems overconservative to revert to the heavy types of stiffening trusses which were used in the early 1900's. If such conservatism is substituted for scientific analysis, forty years of progress in suspension bridge design will have been lost.

ALEXANDER KLEMIN.[28]—By sheer virtue of necessity, aeronautics leads the field in its knowledge of aerodynamics and in its powerful methods of attacking problems of oscillation. For example, in the study of wing flutter, the aeronautical engineer considers the following factors: Mass, moment of inertia, elastic resistance to distortion, gravitational resistance to displacement, internal friction of the material, aerodynamic forces, and damping moments and their changes with vertical and angular velocities. He must make full use of the wind tunnel, in force and oscillation tests, using specially constructed models which follow the laws of dynamic similarity. When he has written down the long, complicated equations, he finds that they cannot be solved because aerodynamic forces vary as the square of the velocity, and the differential equations are no longer (alas) linear differential equations with constant coefficients. He then introduces the powerful idea of "resistance derivatives," which render the equations tractable. When attempts are made to deal with aeronautical oscillation problems by "short-cut methods," these methods fail. It is apparently essential to make a complete, recondite analysis, to take all factors into account, and to study "coupling," which is the bugbear of electrical as well as of aeronautical engineering.

From the writer's reading of the subject of bridge oscillations, he has the impression that civil engineers have tried to solve such problems by simplified methods, without sufficient aerodynamic data, and have failed.

The great merit of this paper lies in the fact that the author has boldly, and with originality, adapted aeronautical methods, both experimental and analytical, to the problem of bridge oscillations. The writer is satisfied that the methods which proved sound in aeronautics have been correctly and carefully adapted in this paper. Thus, perhaps for the first time in engineering literature, a paper has provided a sound basis for the study of bridge oscillations.

ABRAHAM SLAVIN,[29] M. ASCE.—The subject matter in this paper is essentially related to aerodynamic stability of bridges, although it is completely generalized for applicability to other types of section. The author had previously developed simple criteria and formulas for determining the aerodynamic stability of suspension bridges.[30] These simple criteria are of particular sig-

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[30] "Rigidity and Aerodynamic Stability of Suspension Bridges," by D. B. Steinman, *Transactions*, ASCE, Vol. 110, 1945, p. 439.

nificance and considerable practical value.[31,32,33] The paper under discussion is a continuance of this pioneering work and a notable contribution to engineering science. It represents creative research work in solving a new and critical problem with which the profession has been dramatically confronted. For full appreciation of the analytical and practical significance of this important contribution, it should be recognized that it is the first complete theory of aerodynamic oscillations which has the following pertinent features:

(*a*) It is not limited to streamline airfoil sections and idealized thin plates but is applicable to all bridge sections of whatever type or form, including flat plates, girder-stiffened sections, truss-stiffened sections, and all modified or combination sections;

(*b*) It is not limited to coupled oscillations (as in airplane flutter) but also covers vertical oscillations and torsional oscillations (which are the known forms of bridge instability);

(*c*) It is not limited to determining critical flutter velocity but also determines and predicts critical velocities and, in addition, negative damping, rate of amplification, limiting amplitudes, and amplitude response at all wind velocities;

(*d*) It does not require oscillating model tests, which are costly and time consuming, but instead determines all necessary constants of any section by simple static tests on small-scale section models; and

(*e*) It is independent of the known defects and inconsistencies of conventional aerodynamic theory.

Those who have sought to apply conventional airfoil theory to bridge sections have overlooked the fact that the conventional theory assumes rounded leading edges and sharp, tapering trailing edges, and any attempt to apply the theory to bridge sections or even to thin flat plates introduces material errors in the critical region in the vicinity of the leading edge. It should be evident that such reasoning, although perhaps excellent for mathematical exposition, does not apply to the structural sections in bridges. The nonstreamline sections in bridges do not fit the classical derivation, in aerodynamic theory, of the behavior of the streamline sections.

In this paper, the author has combined aerodynamic science, vibration theory, and bridge dynamics, to create a new scientific approach to the problem of bridge aerodynamics. The resulting contribution is a technical achievement that is of the highest practical usefulness.

CHARLES W. DOHN,[34] JUN. ASCE.—These studies of aerodynamic theory and stability as related to bridge oscillations, and particularly as related to suspension bridge oscillations, are stimulating. They point a way toward the design of aerodynamically stable bridges with a minimum of material for maximum stability. However, many engineers (and even those who may have frequent occasion to design such bridges) will probably feel hard pressed to

[31] "Rigidity and Aerodynamic Stability of Suspension Bridges," by D. B. Steinman, *Transactions*, ASCE, Vol. 110, 1945, pp. 572–574.

[32] *Ibid.*, p. 575, Eq. 177.

[33] *Ibid.*, p. 576, Eq. 178.

[34] Engr., New York, N. Y.

follow all the masterful mathematics involved and will look to the author for the simplifying concluding recommendations or specifications.

The method of predicting aerodynamic behavior as described, although largely analytical, requires tests on section models. Data are presented for a section consisting of side girders and solid roadway with a ratio of $d/b = 0.20$, and also for a section that is essentially a flat plate with a ratio of $d/b = 0.02$. The former section proves to be an unstable section and the latter section a stable one. It is probably not a widely appreciated fact that truss systems can also be aerodynamically unstable and, hence, that data demonstrating stability and unstability of truss sections would be particularly pertinent. Would data for girders with $d/b = 0.20$ correspond to truss sections with $d/b = 0.40$, as might be inferred from the author's previous paper[35] presented in 1943? The bulletin on wind-tunnel tests of suspension bridge section models by the aerodynamic laboratories of the Virginia Polytechnic Institute[36] at Blacksburg contains experimental data for a wide range of $\frac{d}{b}$-values for both girders and trusses, and also various slotted and finned sections. This bulletin contains a good explanation of the causes of aerodynamic oscillations and arrives at interesting conclusions. For roadway and girder sections, it appears that sections with $d/b < 0.075$ and 0.05 for deck girders are both torsionally and vertically stable. For greater $\frac{d}{b}$-values, lateral slots and fins are recommended to eliminate torsional and vertical instability, and many practical stable aerodynamic sections are presented.

The nature and distribution of the basic pressures which are the cause of the phenomenon of aerodynamic oscillation are illustrated in Fig. 25. Although

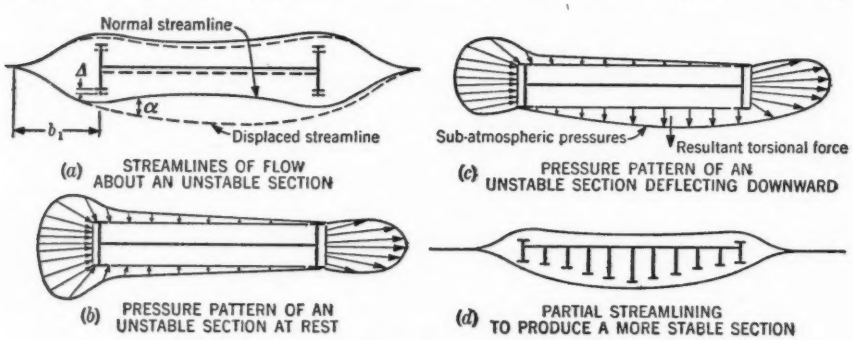
Normal streamline
Δ
α
b_1
Displaced streamline
(a) STREAMLINES OF FLOW ABOUT AN UNSTABLE SECTION
Sub-atmospheric pressures
Resultant torsional force
(c) PRESSURE PATTERN OF AN UNSTABLE SECTION DEFLECTING DOWNWARD
(b) PRESSURE PATTERN OF AN UNSTABLE SECTION AT REST
(d) PARTIAL STREAMLINING TO PRODUCE A MORE STABLE SECTION

FIG. 25.—AIRFLOW ABOUT BRIDGE SECTIONS

they are not conclusive, verified, or complete by any means, they are illustrative of how a movement Δ of the H-section, in Fig. 25(*a*), causes a displacement of the streamlines and a change in the pressure pattern from that in Fig. 25(*b*) to that in Fig. 25(*c*), thus amplifying the movement or deflection and building

[35] "Rigidity and Aerodynamic Stability of Suspension Bridges," by D. B. Steinman, *Transactions*, ASCE, Vol. 110, 1945, p. 458.

[36] "Wind Tunnel Tests of Suspension Bridge Section Models," by F. J. Maher, D. Frederick, E. R. Estes, and D. B. Steinman, *Bulletin of the Virginia Polytechnic Inst., Eng. Experiment Station Series No. 69*, Blacksburg, Va., September, 1948.

up oscillations. For shallow girders and wide roadways the effect of deflection Δ on angle α and on the pressure pattern is proportionately less. An outside projection or cantilever increases the distance b_1 (Fig. 25(c)) so that any deflection will cause a relatively smaller angle α.

In the case of an unstable section, such as that illustrated in Figs. 25(a), 25(b), and 25(c), a reduction of pressure on the one surface, due to streamline displacement, is greater than any change in pressure on the other surface. The net induced force is in the direction of the movement or the deflection of the section, and oscillations are built up or amplified. The limiting amplitude of these oscillations may now be determined from wind-tunnel tests and the presented theory. The streamlines of flow at different distances from the section will be deflected in varying amounts and it is difficult to evaluate the mass of air undergoing a change of velocity and direction in a given time. Furthermore, eddy currents are formed in the wake of the section, altering the nature of the circulation of air about it; in addition, friction losses as well as turbulence are present. Hence, wind-tunnel tests are necessary for quantitative results. The study of the nature of the flow about the section, however, serves as a qualitative guide to design.

In the case of deck girders the vertical difference in pressure due to movement is accentuated by the greater change in curvature of the streamlines, and, hence, the deck girder type of section is more unstable for a given ratio d/b. Fig. 25(d) illustrates a way to obtain streamlining of a limited nature by varying the depth of roadway stringers to obtain stiffening action. This section followed from a direct study of streamlines and would not otherwise be apparent. It is presented as merely suggestive of a qualitative method of section design which can be checked quantitatively by wind-tunnel tests and calculations of limiting amplitudes for a specific design, following the aerodynamic theory of bridge oscillations as presented.

It appears that, from the standpoint of aerodynamic stability, the best roadway section for a suspension bridge would be a flat plate section. Where ratios of live load to dead load are small and where no further stiffening is required to prevent uncomfortable cable distortions due to live loading, such sections are also aerodynamically stable. In bridges requiring a stiffening girder, such a girder could be separated from the floor system, thus providing a flat plate type of floor system. A separated cable system can also be employed to provide distortion resistance and increased damping resistance—at the same time allowing the use of a flat plate type floor section. These three methods of obtaining a flat plate type floor section are illustrated in Fig. 26, in which Fig. 26(a) represents the dead-weight system; Fig. 26(b), the separated girder system; and Fig. 26(c), the separated cable system. Any combination of all three systems is possible and practicable for extremely long spans. In the interest of obtaining a smaller value of d a composite stringer should be used.

A reference list of notations used with dimensional significances, explanatory notes, and cross references would aid in the study and the application of the theory presented in this paper.

What is meant by "coupled oscillations"? It would appear that, according to Table 5, a flat plate with a ratio $d/b = 0.02$ can exhibit instability due to

coupled oscillations. How serious can this be for a practical bridge section? What are the proper modifications or specifications to be followed in designing a flat plate type roadway section in order to avoid the instability caused by coupled oscillations? Evidently, from the material presented, if the frequency of vertical oscillation is greater than the frequency of torsional oscillation, stability of a flat plate section is secured against coupled oscillations. Can this

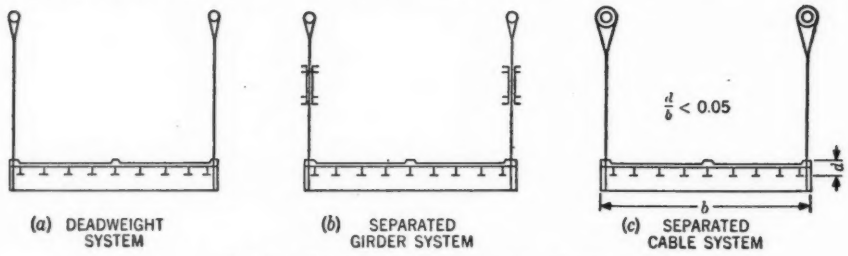

FIG. 26.—FLAT PLATE TYPE ROADWAY SECTIONS

relation be obtained practically, and, if so, how? Can the design of a stay system aid in increasing the frequency of vertical oscillation more than the frequency of torsional oscillation and thus assure stability against coupled oscillations with a flat plate type roadway section? Is the omission of stays in such a case advisable? Would a suspension bridge roadway section, which is stiffer in the longitudinal direction, afford more stability against coupled oscillations?

Returning again to the H-section, as one increases d to obtain added stiffness the value of d/b and the aerodynamic instability of the section increase. What then would be the optimum section to use? From the standpoint of prevention of aerodynamic instability, very small values of $d/b < 0.05$ would appear best, and, for maximum resistance to aerodynamic forces, the highest practicable ratio d/b would seem best. The present tendency (and that of the immediate past) of using $\frac{d}{b}$-values approximating 0.20 for plate girders and 0.40 for trusses is evidently most dangerous and is dictated mainly by "appearance engineering." The first Tacoma (Wash.) Bridge, which was a stiffened plate girder, had a $\frac{d}{b}$-value of about 0.20. The Delaware Memorial Bridge between Delaware and New Jersey, as designed, has a $\frac{d}{b}$-value of about 0.40 with a stiffening truss. Resort to roadway slots and stays can help prevent oscillations with such ratios; however, it would be better either to increase d to obtain added stiffness or to increase b to obtain added stability, or to adopt a section other than an H-section. A relatively heavy dead load will stiffen such a bridge against undue oscillation and the engineers in charge of the design of the Delaware Bridge are relying largely on the dead-weight factor. This bridge has long expensive approaches, and an increase of from 10% to 25% in the cost of the suspended structure is almost a minor item.

Taking into account construction cost indexes, the cost per square foot from back to back of abutment anchorages for suspension bridges has constantly

decreased. Present costs per square foot of deck are less than one half to one fourth of the costs of early long span suspension bridges varying according to length. This decrease is caused partly by the increased efficiency of the construction machinery and partly by the savings in design. Evidently there are practical limits in the reduction of cost. Complete tabulated data on bridges including live and dead loads, all principal structural features, clearance dimensions, roadway sections, cable sections, and special features, plus cost breakdowns, are not generally available. At present it is difficult and costly to assemble such data as interest dictates.

There are evidently optimum relationships between aerodynamic stability and cost per square foot of suspension bridges. The ability to predict in advance what the limiting amplitude of bridge oscillations will be enables a better choice of this optimum relationship. Furthermore with a broader view of the nature of the phenomenon of bridge oscillations it is now possible to give a fuller consideration to designs which eliminate or minimize the occurrence of such phenomenon.

BORIS A. BAKHMETEFF,[37] HON. M. ASCE.—In this paper, which may be considered as the crowning accomplishment of a lifetime endeavor, the author has now united the results of all his previous work into a comprehensive whole. As evidenced by the references cited, the general subject of periodical action caused by agencies of fluid mechanical origin is not new. However, it was necessary to introduce and develop certain novel concepts before the special oscillations of the type covered in the paper could be treated with the fullness and detail required for making the methods directly applicable to practical design.

The subject matter of the problem is necessarily complex and varied. It involves questions of fluid mechanics, elasticity, operational analysis, etc. The paper demonstrates once more, how the former watertight compartments between lines of specialization crumble, and what type of approach is required by present-day engineering science. Mathematical skill is essential, but by far not sufficient. A clear physical perception of the phenomena involved and their dependence on attendant circumstances is equally imperative, and then an ability, sharpened by training and experience, to discern between the essential and the secondary to work out the practical solutions based on simplified premises. The paper reflects these high attainments. The author has deeply deserved the gratitude of the profession.

Certain features in Mr. Steinman's contribution call for special emphasis, especially from the viewpoint of fluid mechanics. First, there is the clear formulation of the fact that instability arises out of "self-induced" pendulations, with the acting force engendered by the linear or angular displacements of the oscillating member and growing with such displacements. In connection therewith, there is the very simple and lucid criterion for appraising the eventual quality of a structural shape, based on the sense (positive or negative) and the magnitude of the "angular gradient" (a term which the writer prefers to that of "slope" used by the author) of the lift or moment characteristic.

[37] Prof., Civ. Eng., Columbia Univ., New York, N. Y.

Finally, there is an experimental technique by which, in tilting a structural member, or curving it, one may determine the eventual aerodynamic response of a shape in its swinging or torsional motion, by way of a static test.

The problems involved in the paper required close cooperation between theory and some specially devised experiments. Once more this procedure, which at times has been termed "semi-empirical," has proved to be so fruitful whenever questions of fluid mechanics are concerned. A part of the experiments was conducted with some assistance, direct or indirect, by the Society's Committee on Research. As chairman, the writer feels gratified that the committee has helped in ploughing a furrow which brought such ample harvest.

There is but one suggestion which the writer would like to make. The breadth and variety of the subject matter encompassed into a short paper make the presentation very brief. For the nonspecialized engineer it is not always easy reading. Could one express the hope that the author might find the opportunity for a detailed treatment of the subject in the form of a book, which would allow the noninitiated engineer to acquire the necessary apprenticeship with greater ease and certainty?

F. J. MAHER[38] AND J. B. EADES.[39]—The paper by Mr. Steinman represents the most significant contribution to date in the field of aerodynamic behavior of suspension bridges. The derivation of the aerodynamic lift and moment equations for shapes such as bridge sections is well nigh impossible by any known theoretical approach. The correctness of Mr. Steinman's approach is substantiated by the correspondence of his results with the equations of theoretical aerodynamics. Actually the equations in the paper are in a more general form inasmuch as the coefficients may be applied to any shapes developing lift forces and moments, whether they be airfoils or bridge sections.

The ingenious use of the concept of the curved model has permitted the author to bring his earlier work to completion. The effects of angular velocity on the lift and moment equations have been reduced to quantities that can be readily determined.

The equations in the paper do not include the effect of the drag component except in so far as the lift and moment curves are corrected for drag. When the section is oscillating either vertically or torsionally the drag force experiences a periodic variation of twice the frequency of the primary oscillation as a result of the changing orientation of the section with respect to the relative wind. This variation is superimposed upon some mean value of drag and would presumably give rise to a vibration (in a horizontal plane) about some mean deflected position. A periodic variation in the forward speed of an airplane (phugoid oscillation) is comparable to this possible motion of the deck. The variable drag force should also cause some additional twisting of the deck particularly if the section is not symmetrical. The total effect of the drag force on the oscillations of typical suspension bridges may be rather small. The writers would like to inquire if any study has been made along these lines and if so what is the approximate magnitude of the effect of the drag force.

[38] Associate Prof. of Applied Mechanics, Virginia Polytechnic Inst., Blacksburg, Va.
[39] Asst. Prof. of Aeronautical Eng., Virginia Polytechnic Inst., Blacksburg, Va.

The development of the complex coefficients is based on the assumption that the phase lag is linear across the width. The writers believe that an expansion of the section, "Effect of Phase Lag Across the Width of the Section" (under the heading, "General Aerodynamic Equations") particularly with regard to the assumption of linearity, would be helpful. Possibly the author could clarify this point in his closure.

It is unfortunate that more test information regarding the dynamic properties of section models is not available to check the theory presented. The check of the results of Louis G. Dunn,[21] cited by the author, provides a significant correlation. The writers have made some tests in the wind tunnel at the Virginia Polytechnic Institute at Blacksburg with the view toward developing a simple method of obtaining the torsional instability graphs for section models. Basically, the method consists of oscillating the section model at a constant frequency in a variable wind stream. The power needed to keep the model oscillating at a constant frequency and amplitude is measured.

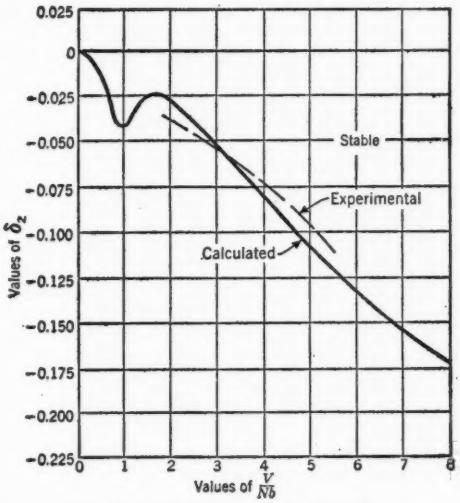

FIG. 27.—TORSIONAL STABILITY GRAPH FOR A FLAT PLATE

Power differences are measured from the base power required to oscillate the system without the model but with an equal moment of inertia. The logarithmic decrement or increment is then computed from the power differences. Such a curve for a flat plate is shown in Fig. 27 as compared with the curve of Fig. 16. In this case the test values were as follows: $d/b = 0.0125$; $b^2/w = 0.151$; amplitude = 2°; and angle of incidence = 0°. Mr. Steinman's results were for the section $d/b = 0.02$ and $b^2/w = 0.268$. Although the values of b^2/w and of d/b are somewhat different, the experimental curve verifies the general shape of the theoretical curve within the limits tested. Curves for the section $d/b = 0.20$ check qualitatively the results of the author with differences in the magnitude of δ and a slightly higher value of the critical value of $V/(N\,b)$ (about 2.9). The experimental method is not as yet refined and is noted here only to show a correlation in the general form of the curves.

The section models used in obtaining the data for the author's calculations were idealized sections without flanges or floor beams and stringers. It was felt that the validity of the general conclusions would not be affected by this idealization. The correlation of the author's results with those of Mr. Dunn seem to bear out this position since Mr. Dunn's tests were made on the actual section model of the Tacoma Narrows Bridge in Washington.

[21] "The Failure of the Tacoma Narrows Bridge," a report to the Hon. John M. Carmody, Administrator of the Federal Works Agency, Washington, D. C., March 28, 1941, by a Board of Engineers consisting of O. H. Ammann, Theodor von Kármán, and Glenn B. Woodruff, Appendix VIII, Fig. VIII-2.

EDMUND PINNEY.[40]—The crucial point of the paper is the "principle of relativity" as stated under the heading, "Use of Stationary Section Models: Use of Curved Models." This is the notion that the flow about a rotating body immersed in a uniform stream is equivalent to the flow about a stationary body whose form is obtained by curving the original body in a suitable manner. The reasoning employed by the author to justify this principle is not sufficiently clear to reveal its essential limitations. A careful analysis shows that it is valid in the case of laminar flow but fails when free vorticity or turbulence is present. In view of the fact that turbulence is a very prominent feature of the flow about oscillating or unstreamlined bodies such as bridges, a theory of this kind is of questionable value.

To support this principle the author mentions the work of G. A. Gourjienko.[10] This relates to the curvilinear flight of airships in which turbulence is a negligible factor. The reference to R. Kassner and H. Fingado[3] in connection with Analogy 2 is evidence against the principle of relativity, and not for it. If this principle were valid, all the terms (not just one) of the air forces of Messrs. Kassner and Fingado would be derivable from it. This is clearly not the case, because their results agree with those of Theodore Theodorsen.[2]

To show that this principle of relativity is invalid when turbulence is present, it is enough to exhibit a single example in which it fails. The simplest such example known to the writer is that of a thin airfoil of zero camber immersed in a uniform stream of velocity, V, and oscillating about its midchord point with an angle of attack $\alpha(t) = \alpha_o \cos(2\pi N t)$, N being the frequency of the oscillation. According to the author (see text concerning Analogy 2), the forces and moments acting on this airfoil should be the same as those acting on a stationary thin airfoil with a camber equal to $b\dot{\alpha}/(8V)$.

The lift and moment expressions for the oscillating airfoil have been derived by Messrs. Theodorsen, Küssner, and many others. They are given by the author (see Table 1, Eqs. 2, and nineteen lines after Eq. 37b) as

$$L = \tfrac{1}{4}\pi\rho b^2 V\dot{\alpha} + \pi\rho b V^2 C\left(\pi\frac{N b}{V}\right)\left(\alpha + \frac{b\dot{\alpha}}{4V}\right) \quad \ldots\ldots (46a)$$

and

$$M = -\frac{\pi\rho b^3 V\dot{\alpha}}{16} - \frac{\pi\rho b^4\ddot{\alpha}}{128} + \tfrac{1}{4}\pi\rho b^2 V^2 C\left(\pi\frac{N b}{V}\right)\left(\alpha + \frac{b\dot{\alpha}}{4V}\right) \quad \ldots (46b)$$

in which $C(k)$ is given by Mr. Theodorsen as

$$C(k) = \frac{H^{(2)}{}_1(k)}{H^{(2)}{}_1(k) + i\,H^{(2)}{}_0(k)} \quad \ldots\ldots\ldots\ldots\ldots\ldots (47)$$

In Eq. 47, $H^{(2)}{}_0$ and $H^{(2)}{}_1$ are Hankel functions of the second kind. Substituting the foregoing expression for camber into the formulas[41] for the lift and

[40] Dept. of Math., Univ. of California, Berkeley, Calif.

[10] "Method of Curved Models and Its Application to the Study of Curvilinear Flight of Airships," by G. A. Gourjienko, *Technical Memorandum No. 829* (Pt. 1) and *Technical Memorandum No. 830* (Pt. 2), National Advisory Committee for Aeronautics, Washington, D. C., 1937.

[3] "Das ebene Problem der Flügelschwingung," by R. Kassner and H. Fingado, *Luftfahrtforschung*, November 20, 1936, pp. 374–387. ("The Two-Dimensional Problem of Wing Vibration," by R. Kassner and H. Fingado, *Journal*, Royal Aeronautical Soc., Vol. 41, 1937, pp. 921–944.)

[2] "General Theory of Aerodynamic Instability and the Mechanism of Flutter," by Theodore Theodorsen, *Technical Report No. 496*, National Advisory Committee for Aeronautics, Washington, D. C., 1935.

[41] "General Aerodynamic Theory—Perfect Fluids," by Theodor von Kármán and J. M. Burgers, in "Aerodynamic Theory" (W. F. Durand, editor), Springer, Berlin, 1935, Vol. 2, formulas (3.1), (4.1), (7.13), (10.8), and the footnote on p. 6.

moment on a cambered thin airfoil:

$$L = \pi \rho b V^2 \left(\alpha + \frac{b \dot{\alpha}}{4V} \right) \ldots\ldots\ldots\ldots\ldots\ldots (48a)$$

and

$$M = \frac{\pi \rho b^2 V^2 \alpha}{4} \ldots\ldots\ldots\ldots\ldots\ldots\ldots (48b)$$

In the notation used by Theodor von Kármán, Hon. M. ASCE, and J. M. Burgers, the camber is $f/(2b)$. In Eq. 47, $C(k) \to 1$ as $k \to 0$, and $C(k) \to \frac{1}{2}$ as $k \to \infty$. It follows that Eqs. 46 and 48 approach each other as $N \to 0$, when turbulence also approaches zero. However, as $N \to \infty$, when turbulence becomes important, $L \to \frac{\pi \rho b^2 V \dot{\alpha}}{2}$ and $M \to -\frac{\pi \rho b^4 \ddot{\alpha}}{128}$ (the leading terms of Eqs. 46); and $L \to \frac{\pi \rho b^2 V \dot{\alpha}}{4}$ and $M = \frac{\pi \rho b^2 V^2 \alpha}{4}$ (the leading terms of Eqs. 48)—exhibiting a profound difference.

The author is primarily interested in applying his principle of relativity to the analysis of unstreamlined shapes like standard suspension bridge sections. This example of the failure of that analysis involves a much simpler streamlined section. Nevertheless the principle, if valid, must apply to both cases; since it fails in one, it cannot be relied upon in the other.

On the other hand, the inconsistency between Eqs. 46 and 48 cannot be blamed on the Kutta condition. This condition was employed in both derivations. If the principle of relativity were valid, they would have led to the same result.

Lack of a high degree of mathematical rigor in dealing with intuitively obvious physical situations may certainly be excused in a paper of this kind. However, in the paragraph preceding Eqs. 2, the author has by-passed some important details of nonobvious fundamental derivations in a manner that needs justification. For example, why should the mass m_a involved in this treatment be the mass of air lying inside the circle circumscribed about the bridge section? Why not less air, or more? Does the author envision the situation as that of a solid cylinder of air rotating in a hole in an otherwise still atmosphere? Such a flow is hydrodynamically impossible. It is true that the terms in Eqs. 2 based on this assumption also appear in Mr. Theodorsen's thin airfoil analysis, but this fact does not establish them in this more general case.

HAROLD A. THOMAS,[42] M. ASCE.—No impartial reader can fail to appreciate the vast amount of thought which the author has devoted to the subject of aerodynamic oscillations of bridges. The essential problem solved in the paper (within a limited range of conditions) is the prediction of the wind-tunnel performance of an oscillating model of a suspension bridge floor and stiffener system from data obtained from wind-tunnel tests on natural-scale and distorted static models of the same section. The analysis makes use of tests on a series of static models which substitute curved surfaces for certain plane surfaces of the prototype, it being assumed that the performance of a straight model in a curved wind can be simulated with satisfactory accuracy by that of a curved model in a straight wind. Another assumption used in the analysis is that the instantaneous pressure pattern on an accelerating surface can be

[42] Prof. of Civ. Eng., Carnegie Inst. of Technology, Pittsburgh, Pa.

simulated with satisfactory accuracy by that on a static surface having the same orientation with respect to the apparent wind direction. This step is a first approximation, which is applicable only when the maximum velocity of every oscillating solid part is small in comparison with the wind velocity, this condition being met satisfactorily in those vibrations of actual bridges where the amplitude of oscillation and the number of oscillatory loops are small. Within the aforementioned limitations, it is not likely that competent readers will question the applicability of the author's assumptions or the correctness of his brilliant mathematical analysis.

It seems probable that the publication of this interesting and technically excellent paper will tend to influence future engineers to employ static models rather than oscillating models in studying the probable aerodynamic performance of specific bridges under design. To maintain a balanced view, the writer would like to comment on certain advantages of oscillating models.

The number of tests that have been made on oscillating models of suspension bridge floor sections in wind tunnels of reasonably large size is very limited, and it is quite possible that such tests, being intended for research as well as for control of specific designs, have been expensive and time consuming. However, the writer has had some experience with oscillating models installed in an air jet 17 in. in diameter, and from this work he can state with assurance that there is no intrinsic reason why the construction and testing of a pair of oscillating models (one for vertical oscillations and one for angular oscillations) should be more expensive or time consuming than the construction and testing of a series of natural and distorted static models of the same scale, equipped with a piezometer system. Mounting a model so that it can oscillate vertically at any desired frequencies is a comparatively simple matter. The same may be said about mounting the model on a pair of ball bearings so that it can oscillate about an axis. More complicated mountings permitting both kinds of oscillations simultaneously should not be required in routine tests for controlling design.

The oscillating models have the advantage of giving the final results instantly, without the necessity for making computations or the possibility of having the work encumbered by arguments on the reliability of various assumptions and analytical processes. The results appear in a direct visual form which is comprehensible with absolute clearness not only by engineers but also by nontechnical men interested in the financial and public welfare aspects of the project. The oscillating models require no piezometer measurements to determine surface pressures. The results obtained on oscillating models are accurate for all amplitudes and frequencies, whereas in analytical methods based on the performance of static models, uncertainty regarding the accuracy of the fundamental assumptions increases with amplitude and frequency, wind velocity remaining constant.

The foregoing remarks on practical model testing are not inconsistent with the writer's admiration of this splendid contribution to a general understanding of the aerodynamic oscillations of bridges.

ELMO G. PETERSON,[43] JUN. ASCE.—It is certainly reasonable to base an analysis of the aerodynamic stability on the pressure distribution graphs of

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the section since the forces causing vertical or torsional motion of a bridge are produced mainly by differences in the pressures on top and bottom of the bridge deck. Since measurements of these pressures on oscillating models have not as yet been obtained, the proper method for correcting the pressure distribution graphs of static models is still unknown. However, the evidence presented (in the numerical examples pertaining to the Tacoma (Wash.) Narrows Bridge, under the heading, "Vertical Instability") for the validity of the proposed phase lag correction (Eqs. 4*a* and 4*b*) shows that this modification of the static pressure distribution graphs is at least of the proper order and magnitude. An earlier publication[44] by the author explains this correction for phase lag. Actual measurements of the pressures on oscillating models would provide the necessary data for confirmation or modification of the theory, but these would be very difficult to obtain with present-day pressure measuring instruments. Perhaps further proof may come only from additional oscillating tests on models.

The writer obtained the static pressure distribution graphs for H-sections, deck sections, and through sections of plate girder bridges, using only straight models and modifying them by incorporating slots of various widths in the bridge deck alongside the plate girders.[45]

The resulting lift and moment curves for the H-sections are almost identical with those already presented.[46] The lift and moment curves for the deck sections and through sections show that these forms of cross section are very unstable but that deck slots do reduce the instability. It is possible that further modifications of the deck section will yield a stable section. There are several psychological advantages of a deck section, besides sightseeing, which warrant additional testing of this nonsymmetrical section to determine if it can be stabilized.

Acknowledgment.—The aforementioned project was sponsored jointly by the Iowa Institute of Hydraulic Research (Iowa City), The Engineering Foundation, and the Committee on Hydraulic Research of the Hydraulics Division, American Society of Civil Engineers.

WILLIAM ALLAN,[47] M. ASCE.—In this paper new working tools for the designer of long span bridges have been developed from basic principles in the several areas of mechanics by painstaking research, intuitively inspired experimentation, clear physical perception, and mathematical skill. Since 1927, a little more than 20 years, the profession has come far from the early wind-force measurements made at Göttingen, Germany,[48] on models of bridge trusses and of trussed bridges with solid floors. There the wind directions were perpendicular to the planes of the trusses and also on the slant laterally and from above, but no displacements were observed.

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[44] "Design of Bridges Against Wind, Part III," by D. B. Steinman, *Civil Engineering*, December, 1945, pp. 559–560.

[45] "An Investigation of the Aerodynamic Stability of Bridge Sections," by Elmo G. Peterson, thesis presented to the State University of Iowa at Iowa City, Iowa, in August, 1948, in partial fulfilment of the requirements for the degree of Master of Science.

[46] "Wind Tunnel Tests Yield Aerodynamically Stable Bridge Sections," by D. B. Steinman, *Civil Engineering*, December, 1947, pp. 747, Fig. 2.

[47] Prof. of Civ. Eng. and Dean, School of Technology, The City College of New York, New York, N. Y.

[48] "Messungen von Brückenträgern" in "Ergebnisse der Aerodynamische Versuchsanstalt zu Göttingen," by L. Prandtl and A. Betz, Oldenbourg, München and Berlin, 1927.

Neither the problem of incipient stability or self-induced oscillations in hydraulics[49] nor that of flutter[11] in aeronautics has the complexity of the oscillation problem concerned with characteristic floor and stiffening member sections of suspension bridges. Mr. Steinman's treatment of stability resulting in Figs. 13 to 16 and Table 5 for selected sections is especially masterful and the results are very valuable.

It seems certain that the paper as printed is an abridgment of the original manuscript material and the writer hopes that the author may one day find it possible to give the profession a more detailed work on the subject and more data and examples on typical cross sections.

PAUL LIEBER.[50]—A rational and quantitative procedure for investigating the aero-elastic stability of suspension bridges is offered in this paper. The theory for nonsteady flow about an oscillating plate developed by Theodore Theodorsen, R. Kassner and H. Fingado, and others has been successfully used for predicting aircraft flutter. In such cases, however, the bodies (airfoils) are streamlined; and, for all practical purposes, the circulation is determined by the Kutta condition. The author correctly points out that the Kutta condition cannot be applied to sections used for suspension bridge construction such as an H-section, for example. Accordingly, it becomes necessary in these cases to determine some of the aerodynamic coefficients experimentally.

The author employs a concept that the effect of curvature of a section model on the flow in a static wind-tunnel test is equivalent to the effect produced by the angular velocity of the section. Thus, the nonsteady forces are obtained from static wind-tunnel tests. These forces are then introduced into the equations of motion, yielding solutions which are in good agreement with some experimental data.

One would expect viscosity effects to be an important consideration in relating wind-tunnel results obtained with stationary section models to the prototypes, especially for sections to which the Kutta condition cannot be favorably applied. The effects of the Reynolds number are not discussed in this paper, and such an investigation is advisable before wind-tunnel results obtained with section models are applied to prototypes.

In the writer's opinion this paper is an important contribution to both civil engineering and to aeronautics.

S. K. GHASWALA,[51] ASSOC. M. ASCE.—The paper forms a significant contribution to the subject of aerodynamic theory of bridge oscillations, and clearly bears the mark of intense creative research and experimentation in a subject which is still in its nebular stage of growth and development. Until comparatively recently, the subject of wind resistance of bridges was relegated to a static sphere, as is evident from the fact that the conventional pressure of from 30 lb per sq ft to 50 lb per sq ft was adopted and always considered safe and justifiable. Only recently did the failures of the Tacoma Narrows Bridge in Washing-

[49] "Zur Theorie des Wasserschlosses bei Selbsttätig Geregelten Turbinenanlagen," by D. Thoma, Oldenbourg, München, 1910.

[11] "Mechanism of Flutter: A Theoretical and Experimental Investigation of the Flutter Problem," by Theodore Theodorsen and I. E. Garrick, *Technical Report No. 685*, National Advisory Committee for Aeronautics, Washington, D. C., 1940.

[50] Asst. Prof. of Applied Mechanics, Polytechnic Inst. of Brooklyn, Brooklyn, N. Y.

[51] Chartered Engr., Fort Bombay, India.

ton (in 1940) and the Chester Bridge in Illinois (in 1944) arouse the bridge engineering profession from slumber by vividly demonstrating the aerodynamic effect of wind in the former failure and the aerostatic effect in the latter. The insufficiency of the conventional methods of wind pressure design were then fully realized and the suspension bridge was thrust into its true classification as a dynamic structure.

The paper affords a clear insight into the subject of oscillation of bridge sections of various types. The methods of classical aerodynamics applicable to perfectly streamlined bodies only have been modified to suit practical bridge sections, and differential equations, which are believed to be the basic formulas governing this new aerodynamic theory of bridge oscillations, have been evolved. By this method, the actual wind pressure design of long suspension bridges of the future will form the acid test of the theory which, as the author claims, is the most complete and most generalized yet presented. In any event, this illuminating paper will deserve the praise of all those capable of appreciating the finer points of analytical research in an uncharted field.

PETER L. TEA.[52]—In designing a new machine as much science as is applicable is used, and also much art, consisting of judgment formed from prior experience. A few machines are built and tested under required conditions, and are then treated roughly to observe how they stand up. Much is thus learned to improve the final design for production.

This convenient process cannot be applied to bridge design. Only one bridge is to be built to suit the expected traffic and conditions at the site, and its cost is very high. Scale models are expensive; they require time to build and to test in large wind tunnels, which involve more expense. In addition, if the model shows marked instability, a new design is required and the process is repeated. Finally, to what degree of accuracy will the model reproduce the oscillations of the actual bridge? A good aerodynamic theory of bridge oscillations produced by the wind has been urgently required.

This timely paper indicates a keen sense of analysis guided by unusual intuition and ingenuity, and an enormous amount of work. Using his theory, Mr. Steinman has made checks indicating good agreement with the performance of a large number of bridges in the United States and abroad.

The procedure calls for static wind-tunnel tests on small-scale test specimens of short length—a very inexpensive operation. The pressure distribution observations are used to obtain a dimensionless form factor y as a function of x along the width b, taken as unity. The y-function could be applied to determine the lift and the torque on a rigid or quasi-rigid bridge of cross section similar to that of the test specimen. The y-function for static conditions is modulated very ingeniously by multiplying y by the phase-lag factor $e^{-i\phi}$ to obtain the shape factor (dimensionless, a function of x) for application to a bridge where there is sinusoidal motion, vertical or torsional, in one of its natural modes From the modulated function the F-curves and G-curves are obtained; and they form a most important part of Mr. Steinman's research, being applied to study the stability or instability of actual bridges (Eq. 13 and 16 and Figs. 15

[52] Associate Prof. of Drafting, The College of the City of New York, New York, N. Y.

and 16). The writer suggests that some wind-tunnel tests be devised (if practicable), under dynamic conditions, to obtain results experimentally which could be predicted by the application of the *F*-curves and *G*-curves, for comparison and discussion.

This paper is a contribution to bridge and structural design of the first order.

EDWARD ADAMS RICHARDSON.[53]—The far from simple aerodynamic theory of flutter has now been reduced successfully to a set of procedures applicable with some ease to practical problems of design in certain important cases. This has been done in part through the use of the pressure distribution over an airfoil in the static case, modified through a simple assumption of validity when differences in phase across the section must be considered. The method of Theodore Theodorsen[11] is based on the integration of the equivalent effect due to a trailing system of somewhat idealized vortices. In part, also, the simplification is secured through simple experiments, cheaply made, on wind-tunnel models of the cross section and on curved modifications thereof—a most important accomplishment.

As these equations have been derived by Mr. Steinman, they apply to the most common type of suspension bridge structure, involving two sets of cables, suspenders, and stiffener girders, or one set for each side of the bridge. They are capable of modification for cases in which the two substantially independent suspending systems are interconnected by transverse suspenders, discussed in an earlier paper,[54] and used in the stiffening of the Deer Isle Bridge in Maine with great success. Such a bridge tends to be free of torsional oscillations although it may exhibit slight lateral swinging with accompanying cyclic variations in angle and in effective wind velocity.

Although most bridges within the range of the given analysis will develop the types of oscillation calculable therefrom, the theory is not developed so as to show that the oscillations may occur. In cases of larger and heavier bridges, or short times of exposure to conditions favoring flutter, it is quite possible that the calculated behavior may not occur. The reasons, therefore, have been discussed previously[55] and need not be repeated. This is a characteristic of all flutter equations, however, not of those particular equations under discussion.

Certain conclusions derivable from the examples should be noted. These examples cover two sets of data: (*a*) Bridge floors with flat plate cross sections and (*b*) those with H-shaped cross sections. Group (*a*) is typical of so-called stable types of cross sections. For them, if combined torsional and vertical oscillations are possible, flutter is possible; but, if either the vertical or the torsional oscillations may be suppressed, then the other type cannot be unstable. This conclusion is consistent with the work of the late Friedrich Bleich,[56] M. ASCE, who used a flat plate bridge floor for study. Group (*b*) differs from group (*a*) by the dangerous instability which can occur not only when there are combined oscillations, but also when vertical oscillations are suppressed, and,

[53] Publications Dept., Bethlehem Steel Co., Bethlehem, Pa.

[54] "Rigidity and Aerodynamic Stability of Suspension Bridges," by D. B. Steinman, *Transactions*, ASCE, Vol. 110, 1945, p. 470.

[55] *Ibid.*, p. 481.

[56] "Report No. 10 on Oscillations of Suspension Bridges with Flat Plate Floors," by Friedrich Bleich, A.I.S.C., New York, N. Y., October, 1945.

with very low degrees of damping, when torsional oscillations are suppressed. Obviously stable types of cross sections are to be preferred. In either case, however, if the torsional oscillations can be suppressed by such methods as those previously described by Mr. Steinman,[54] very moderate damping will insure stable behavior. Hence, it is possible to build relatively slender bridges which are stable, and it becomes a matter of importance to investigate the limitations of such designs through the extension of the analysis to the transverse suspender type of bridge. Likewise, it would be desirable to apply the analysis to such a bridge as the George Washington structure, in New York, N. Y., which has been classified as intermediate between those bridges which are very stable and the other bridges which have shown important oscillations, to determine just what stability may be expected of the structure, the influences of floor type (both present and proposed), and other features.

Mr. Steinman is indeed to be commended for the work he has done in this field.

H. H. Bleich,[57] M. ASCE.—The paper represents an attempt to obtain a solution of the important problem of aerodynamically excited vibrations in suspension bridges. The general dynamic equations of motion can be established and solved without particular difficulty, provided the air forces acting on the vibrating section are known; and the problem confronting the author was, therefore, essentially one of finding these forces.

The author has conceived a novel approach for the determination of the air forces acting on a vibrating section, using tests on static models only. Pressure distribution graphs are obtained, from which phase-correction factors C_1, C_2, C_3, and C_4 are determined by Eqs. 5. In deriving these equations the author found it necessary to make the assumption that the pressures on the oscillating sections are $y_1 e^{-i\phi}$ and $y_3 e^{-i\phi}$, in which y_1 and y_3 are the static pressure distributions. In the writer's opinion, there is no factual basis for this assumption; and, as the quantitative results of the analysis are entirely dependent on these phase-correction factors, the results appear to lack a rational basis.

In the third paragraph of the "Conclusion" it is stated that, for the limiting case of an ideal flat plate, the coefficients in the classical aerodynamic flutter theory and the coefficients determined by the theory proposed in the paper are not identical. The results of the flutter theory check very closely with experiments,[58] thus contradicting the proposed theory for an important special case. The author states:

> "The classical derivation is not applicable to nonstreamline sections, and no method of adapting it so as to fit the known behavior of nonstreamline sections is apparent. The coefficients derived by the simpler concept do fit the known behavior of nonstreamline sections. * * *"

The writer fails to see evidence to support the second part of the statement, and for the following reason: A very important point to be considered when

[57] Associate Engr., Hardesty and Hanover, New York, N. Y.; and Lecturer in Civ. Eng., Columbia Univ., New York, N. Y.

[58] "Use of Generalized Coordinates in Flutter Analysis," by Samuel J. Loring, *Journal*, Soc. of Automotive Engrs., April, 1944, p. 113.

weighing experimental evidence to support any theory is the question of whether the theory to be checked rests on a rational theoretical basis or only on theoretically unfounded assumptions. In the first case one or two examples of agreement between theory and test may be considered reasonable proof; in the second case only a series of tests covering the entire range of application and variables involved can furnish support for the theory. It would appear that the theory presented in this paper belongs to the type described as the second case. Not only is there insufficient evidence for it, but the disagreement with the exact theory supported by tests speaks strongly against it.

It is the writer's opinion that it is not at all possible, as proposed in the paper, to solve this problem using static tests as a sole basis, however desirable this procedure might be; but that only tests on oscillating section models can supply the necessary data.

A first step in this direction was made in 1946 by the late Friedrich Bleich,[27] M. ASCE, in his reports to the Advisory Board on Investigation of Suspension Bridges, Public Roads Administration, where he presented a method for treating catastrophic vibrations, etc.

F. B. FARQUHARSON,[59] M. ASCE.—It would be fortunate indeed if the much simplified procedure advocated in this paper could be proved to be as reliable and as universal in application as is claimed. The writer has hopefully followed the development of this method through its various stages and, although he has found many contributions which assisted his own thinking, it has yet to be shown that this approach is valid.

The most rational check of the validity of the author's predictions, through the use of static section models, would seem to arise directly from laboratory tests on dynamic models. It is the intention of the writer to offer such data for comparison with two of the author's examples. These data were taken from the record obtained from the joint study sponsored by the Washington Toll Bridge Authority and the United States Public Roads Administration where a very extensive investigation has been made on dynamic models of the full bridge as well as dynamic section models.

In Fig. 28, which is a replot of Fig. 13, it would appear that a line drawn horizontally through the appropriate value for the structural decrement δ_s would give a complete picture of the $\frac{V}{N\,b}$-range in which vertical motion would show for that condition of structural damping. Before any motion could be predicted the δ_s-line would have to fall below $\delta_1 = 0.0152$. It would fall below $\delta_1 = 0.0025$ to permit motion in the intermediate stability range and, finally, below $\delta_1 = 0.001$ for motion to show in the lower range.

No evidence has ever appeared in the laboratory which would prove the existence of the lower stability range because of the impossibility of designing a suspension for dynamic section models with $\delta_s = 0.001$ or lower. Similar reasoning would suggest that lower range motion (and probably also intermediate range motion) is also impossible in the field. The author suggests (under the heading, "Vertical Instability: Numerical Examples") that three stability

[59] Director, Eng. Experiment Station, Univ. of Washington, Seattle, Wash.

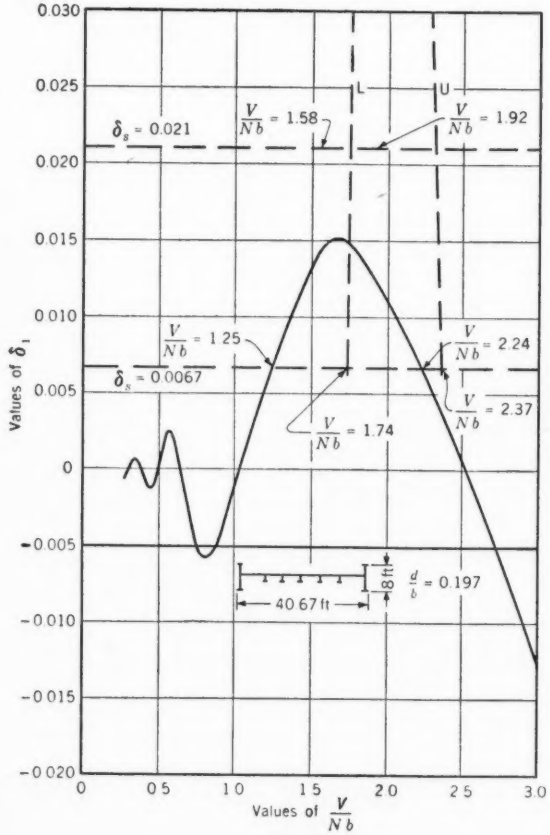

FIG. 28.—VERTICAL INSTABILITY GRAPH FOR AN H-SECTION (d/b = 0.20)

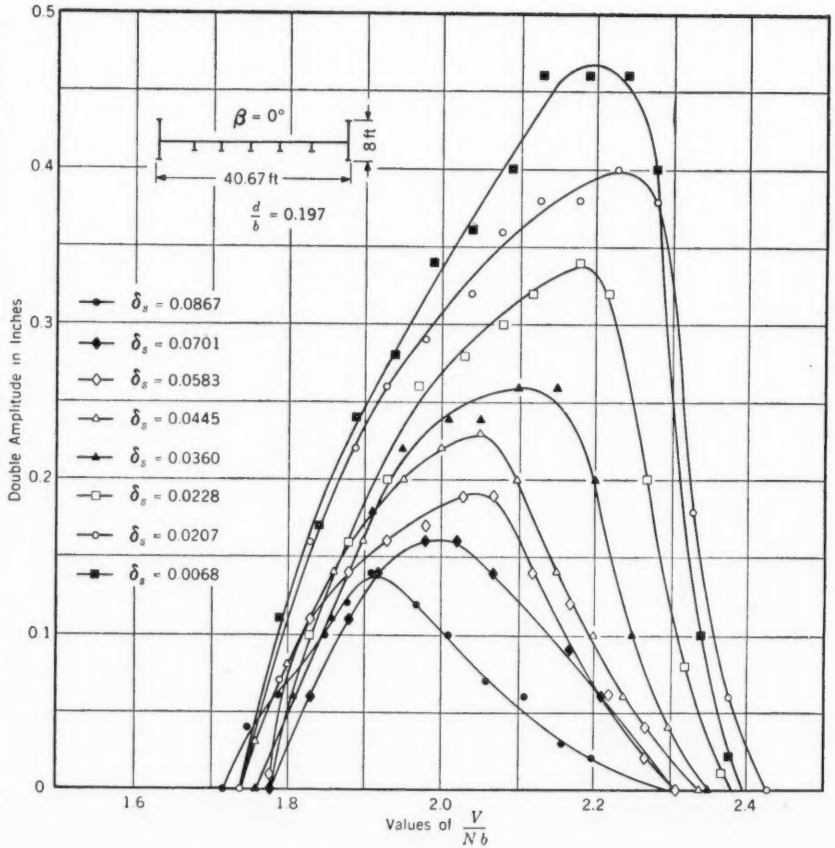

FIG. 29.—OBSERVATIONS ON A DYNAMIC SECTION MODEL, TACOMA NARROWS BRIDGE IN WASHINGTON (CLOSED DECK, NO CURB, AND MODEL SCALE = 1:50)

ranges were evident on the original Tacoma Bridge in the field and cites their boundaries. This information is evidently taken from one of the illustrations of the Carmody report[60] and is entirely unreliable for reasons already pointed out by the writer.[61,62]

Furthermore, it is quite unrealistic to suppose that structural damping involving a decrement as low as 0.0025 could ever be expected in the field on any bridge yet built. The very mild winds which made up the meager record available[61] were so vagrant in nature that the bridge continually wandered from one vertical mode to another and the lack of correlation between velocity readings and observations of the type of motion has led to many erroneous conclusions.

Laboratory tests on an exact replica of the original Tacoma Bridge, with eight different values of structural damping covering the range from $\delta_s = 0.0067$ to $\delta_s = 0.0867$, demonstrated that $\left(\frac{V}{N\,b}\right)_c$ was substantially constant over this range of damping (which was assumed to go well beyond any field possibilities) with an average $\left(\frac{V}{N\,b}\right)_c = 1.75$ (Fig. 29). Vertical motion was apparent in the upper critical region under these conditions of damping, over a range of $\frac{V}{N\,b} = 1.74$ to $\frac{V}{N\,b} = 2.37$.

If a horizontal line is drawn, in Fig. 28, for $\delta_s = 0.0067$, the author's calculations show that the instability range for this structural damping would be from $\frac{V}{N\,b} = 1.25$ to $\frac{V}{N\,b} = 2.24$ (Fig. 28). It is likewise evident that change in damping over the range shown in Fig. 29 involved a negligible (3%) change in the lower critical ratio $\frac{V}{N\,b}$ and a slightly greater (5%) change in the upper critical value of $\frac{V}{N\,b}$. On the basis of these dynamic section model tests the bounding lines in Fig. 28 for the upper critical range should be very nearly vertical, and displaced rather sharply to the right. Thus, the complete results for the eight section model tests with different damping would fit within the two vertical lines L and U if they were extended to cover the range $\delta_s = 0.0867$ which is far beyond the limitations of the author's calculations. It appears that the maximum value of δ_1 (logarithmic increment) in Fig. 28 would have to be multiplied by at least 5.8 to reach a level already demonstrated to be possible on this section.

Tests on dynamic section models at the California Institute of Technology in Pasadena are in equally poor agreement with the author's calculations although they were confined to a single value of structural damping where

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[60] "The Failure of the Tacoma Narrows Bridge," a report to the Hon. John M. Carmody, Administrator of the Federal Works Agency, Washington, D. C., March 28, 1941, by a Board of Engineers consisting of O. H. Ammann, Theodor von Kármán, and Glenn B. Woodruff, Fig. 29.

[61] "Aerodynamic Stability of Suspension Bridges," Pt. I, by F. B. Farquharson, *Bulletin No. 116*, Univ. of Washington Eng. Exp. Station, Seattle, Wash., 1950, pp. 89–99.

[62] "Rigidity and Aerodynamic Stability of Suspension Bridges," by D. B. Steinman, *Transactions*, ASCE, Vol. 110, 1945, p. 493.

$\delta_s = 0.021$. At this damping, represented by the upper horizontal line in Fig. 28, oscillations were recorded over a range of from $\frac{V}{N\,b} = 1.58$ to $\frac{V}{N\,b} = 1.92$. These tests were made on an idealized model consisting of a symmetrical section without girder flanges, sidewalk curbs, or stringers ($d/b = 0.205$). This is the same type of section on which the author has made his static section model tests, which form the foundation for his theory. Laboratory experience has demonstrated that details of the type neglected in the idealized model are often of great importance on this type of section where the actuating forces depend primarily on the distribution of vorticity in the wake.[26]

It is significant that the laboratory tests on this idealized dynamic model have indicated values for the logarithmic increment well above the maximum of $\delta_1 = 0.0152$ revealed by the author's calculations. It should also be noted that vertical motion in the intermediate instability range was observed in these tests where $\delta_s = 0.021$.

In the many full model tests run in the laboratory it has always been observed that the range between the upper and lower critical values of $\frac{V}{N\,b}$ for vertical motion was greater than that noted on the section model. Thus, for the full model of exactly the same section as was used in Fig. 29 the instability range at $\delta_s = 0.009$ was from $\frac{V}{N\,b} = 2.06$ to $\frac{V}{N\,b} = 3.0$.

It seems reasonable to the writer to assume that the author's approach may be more nearly valid for sections stiffened by trusses where the forces arising from vorticity are probably a negligible factor.

The author's characterization (under the heading, "Vertical Instability: Numerical Examples") of the lower instability range as "subharmonic" does not seem to be in accord with the mechanism of vortex excitation. As is well known,[63,64] the relationship which governs the frequency of vortex discharge about an object immersed in a flow is given by the expression:

$$f = \frac{V\,S}{d} \qquad (49)$$

in which V is the wind velocity; d is the depth of the object (measured normal to the flow); and S is the Strouhal number characteristic of the shape of the object.

It was demonstrated at the California Institute of Technology that $S = 0.187$ (approximately) for an idealized model of the original Tacoma Bridge; and it was observed at the University of Washington in Seattle on a full model of the original bridge that the $(3 - N\,V)$-mode appeared twice—once in the intermediate range of instability and, again, at approximately twice the wind velocity, and with much increased amplitude in the high range of instability. It was also observed that the frequency was the same in each appearance and the critical velocity in the high range was 3.2 ft per sec. Thus, using $S = 0.187$, the frequency of the vortex is 3.75 cycles per sec which is close to twice the value

[63] "Uber den Mechanismus des Flüssigheits und Luftwiderstandes," by Theodor von Kármán and H. Rubach, *Physkalische Zeitschrift*, January 15, 1912, p. 49.

[64] "Some Experimental Results on Wing Flutter," by William Bollay and Charles D. Brown, *Journal of the Aeronautical Sciences*, June, 1941, p. 317, Fig. 11.

of the recorded frequency of oscillation. For subharmonic excitation to occur[65] it is only necessary for the exciting frequency to approximate some multiple of the natural frequency of the structure.

Since V_C for the same mode in the intermediate range was approximately one half that in the high range, it is clear that this excitation was harmonic. Obviously the author has reversed the terminology in describing these ranges.

Comparison of Laboratory Data With Numerical Examples (Under the Heading, "Torsional Instability").—Fig. 15 is misleading in two respects: First, an accurate rendering of Eq. 16 would cause the calculated curve to intersect the experimental curve at $\frac{V}{N\,b} = 7.15$ and $\delta_2 = 0.30$. These two curves are parallel only over a limited range. Second, the author seems to have made an error in sign in the calculation of his suggested correction factor to bring the experimental results into harmony with his calculations. Fortunately, all the data necessary to calculate the experimental curve are readily available.[21]

This model was mounted on a somewhat complicated suspension which involved a section of cable from which the model was hung by suspenders. The ends of the cables were restrained by coil springs mounted outside the tunnel. Three measurements were taken at zero wind velocity to isolate the value of δ_s as follows:

$$\delta_s = \delta_3 - \delta_2 + \delta_1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (50)$$

in which δ_1 is the decrement for springs alone; δ_2 is the decrement for springs plus section; and δ_3 is the decrement for springs plus section plus suspension. The record shows that $\delta_s = 0.0299$ and a careful recalculation of the experimental curve plotted in Fig. 15 shows it to be accurate without the suggested added correction to account for residual damping.

However, the critical value of $\frac{V}{N\,b}$ yielded by this experimental curve at zero damping is in serious error for reasons not entirely clear. Many tests run at a later date on a simplified suspension at both the University of Washington and the California Institute of Technology have revealed much lower values for $\left(\frac{V}{N\,b}\right)_C$. The author is in error in quoting the results of tests from the California Institute of Technology run in 1945 and the value quoted is also inaccurate. No tests were run in this laboratory after April, 1943, and the value of $\left(\frac{V}{N\,b}\right)_C = 2.5$ $\left(\text{for } \frac{d}{b} = 0.2\right)$ does not appear in any of the records from that institution. In the course of a series of studies on the effect of damping on the torsional mode, $\left(\frac{V}{N\,b}\right)_C = 1.9$ was reported at zero structural damping and $\frac{d}{b} = 0.2$. At $\delta_s = 0.0299$, $\left(\frac{V}{N\,b}\right)_C = 2.5$.

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[65] "Mechanical Vibrations," by J. P. Den Hartog, McGraw-Hill Book Co., Inc., New York, N. Y., 1947 pp. 448–449.

Dynamic section model tests at the University of Washington have shown that $\left(\frac{V}{N\,b}\right)_c = 2.01$, at $\delta_s = 0.0019$, and $\frac{d}{b} = 0.2$, with no evidence that tends to lend any support to the author's calculated $\left(\frac{V}{N\,b}\right)_c = 2.5$ at $\delta_s = 0$.

Mr. Steinman's dependence on static section model tests in his empirical-theoretical approach to this problem does not hold up, in the case of the structure stiffened by plate girders, in either vertical or torsional motion. The writer has not found the time to explore its feasibility on truss-stiffened structures where vorticity is probably of much smaller significance.

Extensive full model tests run in comparison with dynamic section model tests have demonstrated the validity of the dynamic section model approach to this problem; those who advocate sole reliance on static model tests have yet to prove their case.

GEORGE S. VINCENT,[66] M. ASCE.—There is a significant relation between Mr. Steinman's analysis and that developed by the late Friedrich Bleich, M. ASCE, in 1945.[27] Both follow the Theodorsen analysis of flutter of a thin flat plate[2]—omitting, however, the effect of the aileron and neglecting the inertial forces of the surrounding cylinder of air. They differ in that Mr. Bleich considers only a horizontal or slightly angled wind, he uses the Theodorsen basic flat plate forces, and then he introduces, as a corrective term, an equivalent lift force derived from dynamic wind tests of a section model of the bridge. The author, on the other hand, includes a procedure for covering the effect of an initial angle of incidence and he builds up the force terms by data derived from static wind tests on normal and curved section models, thus requiring no corrective term to fit the analysis to the actual section (provided the static test data are adequate for the purpose). Furthermore, he uses the lift force and torque force terms separately to derive predictions for pure vertical and pure torsional oscillations.

The best available method for appraising the analysis lies in correlating it with the results of the three-dimensional aerodynamic model tests made at the University of Washington (in Seattle), cooperating with the Washington Toll Bridge Authority, the United States Bureau of Public Roads, and the university.

The treatment of the effective angle of incidence, α_i, must be extended for a cambered bridge. Eq. 11*f* expresses the angle, α_d, through which the structure will be rotated by the static wind forces—that is, the lift and torque acting at the midpoint of the width, when they are resisted by the over-all torsional rigidity, K_2, of the structure. If the bridge is cambered, the part of the horizontal static wind force resisted by the truss is carried to the truss supports several feet below the line of application of the wind force to the truss, resulting in an overturning moment which tilts the leeward side downward. On the full model of the new Tacoma Narrows Bridge (in Washington), with a 2,800-ft vertical curve between grades of from + 3% to − 3%, this effect of camber was about twice the effect of the action represented by Eq. 11*f* as depicted by the

[66] Prin. Highway Bridge Engr., Bureau of Public Roads, U. S. Dept. of Commerce, Structural Research Laboratory, Univ. of Washington, Seattle, Wash.

section model behavior, and was of opposite sign. Thus, the resultant full model pitch as determined by both factors was about equal to that of the section model but opposite in direction. It could be neglected for small angles of incidence, with prototype velocities less than 60 miles per hr, but rose from 1° to $2\frac{1}{2}$° for higher velocities and greater angles of incidence.

The overturning wind moment not only depresses the leeward side but also increases the tension in the leeward cable, thus increasing its spring constant without altering the mass suspended from it. This action increases its natural frequency while correspondingly decreasing the natural frequency of the windward cable, which might be expected to affect the oscillation of the bridge in the wind. A test was made to determine this point. The wind shoes were loosened, permitting the suspended structure to swing downwind and relieve the truss of all wind load since it had no wind reaction. The shoes were then fastened in the unstressed position to steady the structure and the routine tests were made. The change in behavior was insignificant.

The author indicates that the curves for δ, the logarithmic increment due to wind action, are parabolic when plotted against the amplitude. A considerable number of these curves has been plotted directly from aerodynamic tests on both full models and section models and for sections subject to pure vertical oscillation and those subject to flutter, the structural damping having been determined by other tests and eliminated. Some of the curves are approximately parabolic but more of them are practically straight or have pronounced reverse curvature. For the same section and angle of incidence they often show a progressive change as the wind velocity is increased.

The numerical examples illustrating coupled oscillation (flutter) have more significance than is implied by the statement that the data are assumed. With the exception of the s-values and the q-values all the assumed data are identical to the properties of the prototype of the full model of the Tacoma Narrows Bridge as used and identified by Mr. Bleich in his illustrative problem.[67] The vertical and torsional frequencies are the same and the assumed equations for the wave forms are those calculated by Mr. Bleich for that structure. (It should be noted that the author's value of b is twice that of Mr. Bleich and his value of μ is $\frac{2}{\pi}$ times that of Mr. Bleich.)

Both analyses treat the bridge as a thin, flat plate which assumes that the truss members have a negligible effect on the air forces. Mr. Bleich's solution for the critical velocity agrees quite well with the curve from the full model tests.[68] The velocity computed by Mr. Steinman is about half as great and corresponds to the test results for an angle of incidence of + 3° (the sign convention for the angle of incidence being the reverse of that used by Mr. Bleich[68]). As the author indicates, the effect of an angle of incidence is to determine the point on the curves at which the slopes are taken. Figs. 5 and 6, from which the slopes are taken for the examples, show constant slopes between the angles of ± 5° or 6°; hence, throughout this range of the angle of incidence the solution would be the same as indicated—that is, 69 miles per hr.

[67] "Dynamic Instability of Truss-Stiffened Suspension Bridges Under Wind Action," by Friedrich Bleich, *Transactions*, ASCE, Vol. 114, 1949, p. 1202.

[68] *Ibid.*, p. 1208, Fig. 12.

For this numerical example (the new Tacoma Narrows Bridge but without slots or bottom laterals) it appears that the flat plate analysis will predict the flutter velocity quite well without correction, possibly because of the skeleton construction of the sidewalk stringer at the leading edge. Other truss-stiffened sections tested have required the corrective term.[69]

Mr. Bleich's analysis has the advantage that the corrective term is introduced at the end and used in only the final steps, whereas the author's experimental values, which provide the adaptation to the section under investigation, are subjected to several successive processes. In each of these processes simplifying approximations are made, such as the idealized expressions for the slopes of the lift and torque curves and the use of the "δ-parabolas." The effects of these are probably cumulative. Furthermore, the static models are idealized, as evidenced by the perfect symmetry of the lift and torque graphs for positive and negative angles of incidence. Most tests on models of actual bridge sections show that these curves are not symmetrical but are materially modified by the influence of the stringers and, sometimes, the curbs, and that they are affected by the girder flanges. A few such curves have been published.[70]

Another handicap is the reliance on the static model tests for the air force terms. If the lift and torque graphs have pronounced negative slopes it may be expected that the structure will oscillate and that, with increasing wind velocity, the amplitude will increase indefinitely as is recognized by Eq. 19*b*. This tendency is explained by the "negative slope theory" proposed by J. P. Den Hartog as an explanation of the "galloping" of transmission lines.[71] It does not explain the successive restricted modes, each of which prevails over only a limited range of velocity. The phase functions, applied to the static forces, are also not entirely adequate. The vortex action, not revealed by the static tests, superimposes upon the static pressure pattern a small periodic variation which, when resonant with a natural frequency of the structure, assumes an importance far beyond its relative strength. Furthermore, as clearly indicated by smoke stream tests, the vortex action becomes much stronger once oscillation begins and its frequency is controlled by the motion of the structure over a limited velocity range.

Perhaps one reason for underestimating the importance of the vortex is that, on streamlined sections, the vortex is shed from the trailing edge and exerts no continuing force on the body. On the other hand, the vortex is shed from an abrupt corner of the leading edge of a bridge section and sharply modifies the velocity pattern and the pressure on the structure as it moves across. Vortex action is not so vigorous on truss sections because of shallower depths in the vicinity of the deck, and, therefore, the flat plate analysis gives fair predictions of the critical velocity of the flutter characterizing such sections. Vortex action lowers the critical velocity.

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[69] "Dynamic Instability of Truss-Stiffened Suspension Bridges Under Wind Action," by Friedrich Bleich, *Transactions*, ASCE, Vol. 114, 1949, pp. 1214–1218.

[70] "Aerodynamic Stability of Suspension Bridges with Special Reference to the Tacoma Narrows Bridge," Pt. I, Investigations Prior to October, 1941, by F. B. Farquharson, *Bulletin No. 116*, Univ. of Washington Eng. Experiment Station, Seattle, Wash., Chapter 7, pp. 60–68.

[71] "Mechanical Vibrations," by J. P. Den Hartog, McGraw-Hill Book Company, Inc., New York, N. Y., 1947, Article No. 59.

The writer recommends the use of the flutter analysis for the preliminary examination of proposed designs and as a method of surveying the probable influences of alternative dimensions, weight distribution, and elastic properties. For the most reliable investigation of the aerodynamic behavior of the preferred design (aside from costly full model tests) he advocates dynamic wind-tunnel tests on section models mounted on free springs to produce the proper vertical and torsional frequencies. These tests will reveal, quite reliably, the critical velocity of any catastrophic oscillation and will yield data on energy transfer (as measured by the logarithmic increment) which can be applied to the prototype by a single integration process. This must be used with the best available analysis of the structural damping of the bridge and with due consideration of the velocity, uniformity, and steadiness of the wind at the site.

There is strong evidence that at least one important suspension bridge would undergo objectionable oscillation except for the favorable effect of the terrain on the wind. It is not likely that this is the only case of the kind. At the present stage of knowledge there is some risk in assuming that a proposed design will be satisfactory simply because it resembles a satisfactory existing bridge.

D. B. STEINMAN,[72] M. ASCE.—To all who have contributed discussions, the writer is deeply grateful. The preparation for this work necessarily included an exhaustive analytical and critical study of the ponderous literature of airplane flutter theory. After checking and collating the involved mathematical analyses of the various writers on the subject, the writer was forced to the conclusion that those theories do not offer a directly applicable solution of the problem of bridge oscillations, and that existing aerodynamic theory is inadequate for a solution of the problem. New creative thinking was necessary, with a courageous departure from the limitations of prior conventional theory.

The mathematical derivation of the conventionally accepted theory for airplane flutter is so abstract, involved, and indirect that the physical picture is lost from view, and fallacies of assumptions and gaps in the physical reasoning may be buried under the mountains of mathematics. Few understand the derivation, and still fewer have even tried to check the analysis. Nevertheless, proud reference to the theory has become a cult, and any challenge or departure is attacked as heresy.

The Concept of Curved Models.—The outstanding new concept beyond the writer's previous publications on the subject is the use of curved section models. This was an inspiration. It supplied the missing link in the treatment of angular oscillations. It formed a perfect parallel to the accepted use of tilted section models to represent vertical oscillations. It was a clearly logical extension of the same physical and mathematical thinking. The concept of curved section models represents a relativity principle. It corresponds to a transformation of coordinates, including space and time, to yield a "curvature of space."

The addition of curved section models to the writer's analysis supplied the previously missing terms for complete correspondence with established aerodynamic theory for airfoils. It also supplied the missing factors for complete correspondence between the writer's formulas and the known data from large-

[72] Cons. Engr., New York, N. Y.

scale wind-tunnel tests on bridge models. In addition, the writer made an independent check by comparing the established theoretical formulas for curved thin plates with the corresponding established formulas for flat plates in angular oscillation. The concept of curved section models has been checked, verified, and confirmed in a hundred different ways. The writer is thoroughly convinced of its basic validity.

Because the idea seemed so startlingly new, the writer expected discouraging criticism. It required courage to present it to the profession. This was done in a paper[73] before the Iowa Hydraulics Conference in 1947. Instead of meeting with adverse criticism, the reception was most encouraging. The writer, however, was somewhat disappointed to learn that the idea was not entirely new.[10] He subsequently found that the concept is well known and established in the more modern literature of airfoil analysis, under the name of "dynamic camber"; under this name, the curved thin plate analogy is used and accepted as a short cut for writing the formulas for airfoils in angular oscillation.[74] The writer was the first to conceive and apply the idea as an actual test procedure in the study of airplane flutter and bridge oscillations.

The curved model concept, as applied to an idealized thin plate, checks identically the "quasi-steady force terms"[26] in the theoretical formulas for oscillating airfoils. No claim is made that the curved model yields all the terms in the complete theoretical formulas; the centrifugal force term (easily added) and the acceleration term (negligible) are written separately. The criticism that the curved model does not duplicate all the terms of the complete theoretical formulas represents a lack of understanding of the concept and of the underlying theory. The sole purpose of the static models, both tilted and curved, is to supply the terms that cannot be written by theory. In the application to bridge oscillations, the acceleration terms are written by established aerodynamic theory for thin plates, with simplified extension of the derivation to other sections. Since these acceleration terms are negligible and are, in fact, neglected, challenging the mathematical rigor of their evaluation is hairsplitting criticism.

Furthermore, it is not claimed that the static model tests give the forces corrected for "phase lag." In the terminology of conventional aerodynamics, this correction is identified with "vorticity distribution in the wake." The correction for phase lag or vorticity, whichever name is preferred, is represented by writing the correction factor as a coefficient C in all of the quasi-steady force terms in the formulas.

Vorticity.—In the interests of clear physical thinking, a common misconception needs correction. That misconception is represented by the use of such phrases as "vortex buffeting" or "forces produced by vorticity." These phrases represent a mathematical device, not a physical action. In the repeated use of these phrases, the fact that they are only a mathematical device has been forgotten. The oscillations of a section, even in the case of the forced vibrations

[73] "Problems of Aerodynamic and Hydrodynamic Stability," by D. B. Steinman, *Proceedings*, 3d Hydraulics Conference, *Bulletin No. 31*, State Univ. of Iowa Studies in Eng., Iowa City, Iowa, 1947.

[74] "An Investigation of the Conditions for the Occurrence of Flutter in Aircraft and the Development of Criteria for the Prediction and Elimination of Such Flutter," by Albert E. Lombard, Jr., thesis presented to the California Inst. of Technology at Pasadena, Calif., in 1939, in partial fulfilment of the requirements for the degree of Doctor of Philosophy.

of a cylinder, are not caused or produced "by the vortices." The vortices in the wake are merely counters, markers, or footprints providing a convenient physical and mathematical "trail" from which the circulation about the section and the consequent forces acting on the section may be inferred, formulated, and computed. The limitations of classical aerodynamics necessitate this backhanded approach. From footprints, a crime may be reconstructed. From the "vortex trail," the forces acting on a section and their relative phase may be deduced. The vortices in the wake do not exert a force on the section. Their work was finished at the instant when they were shed into the wake.

Another common error, also associated with the concept of vortex buffeting and arising from the same failure to visualize the physical picture underlying the mathematics, is the notion that the point of shedding of the vortex is the point of application of the related lift force. There is no direct relation between the two. The lift force is determined by the circulation, and the change in circulation is determined only by the strength of the released vortex and in no way by the location of the point of shedding. In a streamline airfoil, the vortices are shed at the trailing edge, but the corresponding lift force acts at or near the forward quarter point. In fact, by conventional aerodynamic theory, the shedding and trailing vortices do not produce any shift of the resultant lift force from its fixed position at the forward quarter point. In a truss or girder bridge section, vortices may be shed at the leading edge, but that does not mean that the corresponding lift force is applied at the leading edge. Any such crude assumption is wholly unscientific and would vitiate an analysis.

The Concept of Phase Lag.—In the interests of clarity, the writer prefers to emphasize the concept of phase lag. Although few may have recognized the fact, the coefficients C in the formulas of the conventional flutter theory are really phase-lag factors. That is why the coefficients C have the value of unity at zero phase lag $\left(\text{infinite } \frac{V}{N\,b}\right)$. That is also why the "apparent mass" terms, representing instantaneous inertia forces, do not have the phase-lag factor C applied to them. Cloudy references to "turbulence" and "vorticity distribution" tend to obscure the true relationship.

In comparison with ice coated wires, or other sections of negligible width (compared with V), the new feature introduced with wider sections is the time required for the fluid flow, or for any disturbance in the fluid flow, to traverse the width of the section. This introduces the new factor—phase lag. A flow disturbance, initiated at the leading edge, takes time to traverse the width and encounters a progressively increasing difference of phase as it traverses the oscillating section. As different points of the width are reached, different stages of the cycle of oscillation are encountered, including differences of velocity and even differences of direction of motion. To cover all cases, the general expression for energy input per cycle is $\Delta W = \pi\, P\, a \cos\phi$, in which P is the force; a is the amplitude; and ϕ is the phase difference between amplifying force and velocity of displacement. The multiplier $\cos\phi$ is the correction factor in multiplying two vectors differing in phase by the angle ϕ, illustrated by the "power factor" applied to the product of volts by amperes in alternating current.

The writer wishes to re-emphasize the importance of his application of the phase-lag factor to the rational aerodynamic analysis of oscillating sections.

Comparison with Conventional Flutter Theory.—Among all the heartwarming comments on this paper, there were only three adverse discussions. Mr. Pinney challenges the writer's concept of curved models by seeking to prove that the resulting formulas for a thin flat plate are not consistent with the formulas of conventionally accepted flutter theory. In this attempt Mr. Pinney has unwittingly made a number of mathematical errors and omissions. These errors are material. In his Eq. 48*a*, the correction for centrifugal force is missing, and in Eq. 48*b*, the angular acceleration term is missing, also the centrifugal force term (which yields zero M only for $C = 1$). In his equation (for $C = \frac{1}{2}$) derived from Eq. 46*a*, the numerical cofficient is wrong and the angle term is missing. In his equation derived from Eq. 46*b*, the angle term and the angular velocity term are missing. In Eqs. 48*a* and 48*b*, the coefficint C should be inserted at all the "quasi-static" terms. If this is done, together with correction of the other cited errors, Eqs. 46 and 48 will be completely identical. Moreover, Eqs. 46 and 48 cannot be expected to remain identical when $C = \frac{1}{2}$ is substituted in the one and $C = 1$ is substituted in the other. The same value of C must be used in the two pairs of formulas for any valid comparison. Confusing references to turbulence should not be allowed to obscure this obvious truth. If these corrections are made, Mr. Pinney's argument vanishes.

The best way to show the consistency of the writer's theory of bridge oscillations with the accepted conventional theory of airplane flutter is by direct application to thin flat plates. This comparison will also bring out the essential differences or points of departure.

For the limiting case of a thin flat plate ($d/b = 0$), or a streamline airfoil, by aerodynamic theory: $s_1 = 2\pi$; $s_2 = \pi/2$; $s_3 = -\pi$; and $s_4 = 0$. With these values, Eqs. 2 reduce to

$$L = \pi \rho V^2 b \left[C_1 \left(\alpha - \frac{\dot{\eta}}{V} \right) + (1 + C_3) \left(\frac{b \dot{\alpha}}{4 V} \right) - \frac{b \ddot{\eta}}{4 V^2} \right] \ldots (51a)$$

and

$$M = \frac{\pi}{4} \rho V^2 b^2 \left[C_2 \left(\alpha - \frac{\dot{\eta}}{V} \right) - \frac{4}{\pi} (C_4 s_4) \left(\frac{b \dot{\alpha}}{4 V} \right) - \frac{b^2 \ddot{\alpha}}{32 V^2} \right] \ldots (51b)$$

Although $s_4 = 0$ for this case, $C_4 s_4$ is not zero (except at zero k). To avoid this indeterminacy, $C_4 s_4$ may be resolved into two component contributions (which cancel at zero k). Since $s_1 = 2\pi$ and $s_3 = -\pi$, the wind direction of zero lift on the cambered model is at $\alpha = \beta/2$. For this condition $C_L = s_1 \alpha + s_3 \beta = 0$, and $C_M = s_2 \alpha + s_4 \beta = \frac{\pi}{4} \beta$, represented by a couple. For such condition of constant zero lift (hence, constant zero "circulation" and constant zero strength of trailing vortex wake), aerodynamic theory yields (or assumes) zero phase lag, or a phase-lag factor of unity. Now, bringing the wind up to the horizontal (subtracting $\alpha = \beta/2$) will add a lift force ($C_L = -C_1 s_1 \alpha = -C_1 \pi \beta$) acting downward at or near the quarter point, which produces a

moment represented by $C_M = -C_2 s_2 \alpha = -\frac{\pi}{4} C_2 \beta$. The total torque on the section in horizontal wind will then be represented by $C_M = \frac{\pi}{4}(1 - C_2)\beta$. Thus, $C_4 s_4$ has been resolved into $\frac{\pi}{4}(1 - C_2)$. To complete the picture, the cambered model correction for centrifugal force is added, represented by $C_L = -\pi\beta$. The total lift due to $\dot{\alpha}$ will then be represented by $C_L = -\pi(1 + C_1)\beta$, replacing $C_L = -\pi(1 + C_3)\beta$.

With these substitutions, Eqs. 51 reduce to

$$L = \pi \rho V^2 b \left[C_1 \left(\alpha - \frac{\dot{\eta}}{V} \right) + (1 + C_1) \left(\frac{b\dot{\alpha}}{4V} \right) - \frac{b\ddot{\eta}}{4V^2} \right] \ldots (52a)$$

and

$$M = \frac{\pi}{4} \rho V^2 b^2 \left[C_2 \left(\alpha - \frac{\dot{\eta}}{V} \right) - (1 - C_2) \left(\frac{b\dot{\alpha}}{4V} \right) - \frac{b^2 \ddot{\alpha}}{32 V^2} \right] \ldots (52b)$$

Conventional aerodynamic theory (by Theodore Theodorsen,[2] R. Kassner and H. Fingado,[3] and subsequent writers) yields the same formulas, with C written instead of C_1 and C_2. For the limiting case of infinite $\frac{V}{N b}$ (or zero k), for which all C-factors are unity, Eqs. 51 and 52 check Mr. Theodorsen's equations identically.

The foregoing method of writing Eqs. 52 from Eqs. 51 depends upon the validity of accepted aerodynamic theory. An extension of the same method to the general case (deeper sections) can be used to eliminate C_3 and C_4 from the general formulas of this paper, leaving only C_1 and C_2. The writer has not done this, because he is not convinced of the complete validity of conventional aerodynamic theory.

In Mr. Theodorsen's analysis, $C = F - i\,G$, a complex function of k, similar to but not identical with, C_1, C_2, C_3, and C_4 of Eqs. 5. The coefficient C represents the phase correction and is derived from the circulation corresponding to the "discontinuity" or vortex sheet, of simple harmonic variation of strength, extending from the trailing edge of the section to infinity. A peculiar feature of the theory is that all lift resultants affected by the multiplier C are invariably applied at the forward quarter point, whether due to angular position, vertical velocity, or angular velocity.

For an ideal flat plate or streamline airfoil, the static effect of α (also representing $-\dot{\eta}/V$) is an upward lift resultant at the forward quarter point. The phase correction, by Mr. Theodorsen's analysis, multiplies this lift resultant by C, without changing its point of application. By Eqs. 51 and 52, the phase correction multiplies this lift force by C_1 and its moment by C_2, representing an indicated shift in its center of pressure. Since the effect of phase difference across the section alters the pressure distribution, it would appear that the C-coefficients should not be the same for lift and for moment but should normally be different, as represented by C_1 and C_2.

Although the writer's F-functions and G-functions are not identical with those introduced by Mr. Theodorsen, certain governing features are common to

both. At the limit of high $\frac{V}{N\,b}$ (or low k), corresponding to zero phase difference, all F-functions approach a common limit of $F = 1$, and all G functions approach a common limit of $G = 0$. At the other extreme of low $\frac{V}{N\,b}$ (or high k), the G-functions have a common limit ($G = 0$), but the F-functions have different limits ($F = \frac{1}{2}$, versus F_1, F_2, F_3, and $F_4 = 0$). For this condition of infinite phase difference or infinite frequency, one would expect that the aerodynamic effect should be vanishing, as represented by $F = 0$. At intermediate values of $\frac{V}{N\,b}$ or k, the two sets of F-functions and G-functions show different laws of variation. The sinusoidal variations of the writer's F-functions and G-functions are more consistent with the known sinusoidal curves of instability response graphs.

It should be noted that the Theodorsen equations are intended to apply only to streamline airfoils with rounded leading edge and tapered trailing edge. It is a violation of the inherent assumptions of Mr. Theodorsen's derivation to apply his equations to a thin flat plate ($d/b = 0$). For deeper nonstreamline sections, the Theodorsen equations are admittedly inapplicable.

According to Theodor von Kármán, Hon. M. ASCE, and J. M. Burgers,[9] a weak point in the idealized aerodynamic theory is that it involves a rough approximation in dealing with the leading edge. Even in the case of an infinitely thin airfoil, the "stagnation point" is not at the leading edge, as assumed, but is some distance along the airfoil. The induced flow velocity turns out to have infinite values at the leading edge, and the neglect of these induced velocities against V results in a material error. For example, the actual pressure distribution graph for a cambered airfoil, as represented by Fig. 8(b), is not a symmetrical curve (ellipse) as derived by classical theory, and therefore does not yield zero moment.

By using actual test data as a starting point, instead of relying wholly on idealized aerodynamic theory of known imperfections, the writer's formulas should represent the closer approach to reality and to generalized validity.

For H-sections and other practical bridge sections, the problem of predicting the aerodynamic forces mathematically appears to be far beyond the resources of conventional aerodynamic theory in its present development. The semi-empirical method as presented in the paper is offered as a practical engineering solution. Unique flexibility in application to all sections (and varying angles of incidence) has been secured by writing the slopes (s_1, s_2, s_3, and s_4) as parameters in the equations; and a simple, practical method, scientifically logical and of indicated validity, for writing the phase-correction coefficients (C_1, C_2, C_3, and C_4) for any section, without limitation, is supplied by Eqs. 5. Classical theory offers no possible method for writing these important coefficients for the general case.

When an engineering problem urgently needs a solution, engineers cannot leave it unsolved merely because existing, established theory is incapable of solving it.

Linearity of Phase Lag.—Messrs. Maher and Eades invite an expansion of the topic of phase lag, particularly with regard to the assumption of linearity.

[9] "General Aerodynamic Theory—Perfect Fluids," by Theodor von Kármán and J. M. Burgers, in "Aerodynamic Theory" (W. F. Durand, editor), Springer, Berlin, 1935, Vol. 2.

The following notes, excerpted from a more extensive analysis by the writer, may be helpful.

An instantaneous (differential) change in the position or motion of a body produces a change in the flow of the surrounding fluid. This change of flow is not propagated instantaneously, but with a finite velocity V of the fluid. As stated by Messrs. von Kármán and Burgers,[9]

> "The action produced by a force is carried along by the general motion of the fluid with the velocity V * * *. In the picture of an airfoil moving through air at rest, the vortices remain at the spots where they are formed."

Accordingly, an aerodynamic disturbance (change of flow) traverses the width b of a section with the flow velocity V, or in the total time $t_1 = b/V$. For an oscillating section, the change of phase per second is $\omega = 2\pi N$. Hence, the total phase difference encountered by the aerodynamic disturbance as it traverses the width of the section is

$$\phi_1 = \omega t_1 = \omega \frac{b}{V} = 2\pi \frac{N b}{V} = 2k \ldots\ldots\ldots\ldots\ldots\ldots (53)$$

As the flow disturbance traverses the width of the section with the velocity V, it will traverse any fraction x of the width b in the time $t = x b/V$. Hence, the phase difference encountered by the disturbance as it traverses the width $x b$ is

$$\phi = \omega t = \omega \frac{x b}{V} = x \phi_1 = 2 k x \ldots\ldots\ldots\ldots\ldots\ldots (54)$$

Note that the flow disturbance traverses the section with linear rate of propagation from the leading edge to the trailing edge. It starts at or near the leading edge at the instant of the differential change in the position or motion of the section. Consequently, at any fraction x of the width of the section, ϕ is the phase lag of the flow disturbance relative to the causative displacement of the section. The local flow disturbance, in turn, instantaneously determines the local change of pressure. Accordingly, the local change of pressure will follow the same harmonic cycle as the oscillations of the section, but with a phase lag of ϕ between them. By Eq. 54, the phase lag ϕ is proportional to x. It is zero at the leading edge and increases in straight-line variation to a maximum at the trailing edge.

The phase-lag factor is $\cos \phi$ for the real terms and $\sin \phi$ for the imaginary terms. These are the multipliers of the quasi-steady pressures along the width of the section, represented by the ordinates y of the pressure distribution graph, as applied in Eqs. 5. In mathematical language, the two multipliers are combined in their equivalent, $e^{-i\varphi}$, as a single multiplier.

Classical hydrodynamics (or aerodynamics) has been devoted primarily to the study of steady states, and the time lag or phase lag arising in nonsteady states is a comparatively new problem.

Those who have studied the conventional theory of airplane flutter may be surprised to learn that the coefficient C in Mr. Theodorsen's formulas represents a phase correction of linear variation across the section but with that variation (paradoxically) reversed in direction. This fact is not apparent from the

formulas. A closer mathematical analysis of the theory reveals that the phase factor for the individual ordinates (corresponding to the writer's phase factor $e^{-i\phi}$) is given by the linear relation:

$$C_x = x + (1 - x)\, C \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (55)$$

in which (using Mr. Theodorsen's notation) x is $+ 1$ at the trailing edge, zero at the midsection, and $- 1$ at the leading edge. Accordingly, the phase-correction factor C_x is unity at the trailing edge (a consequence of the Kutta condition), C at the midsection, and $(2\,C - 1)$ at the leading edge, with a straight line joining these three values and all intermediate values. The overall phase factor C is simply the value at the midsection, or the average value for the entire section. From these values of C_x, the phase lag ϕ is zero at the trailing edge and increases toward the leading edge. This is exactly the reverse of the direction of variation one would expect. In this case, moreover, it is not the phase lag but the phase-lag factor that shows straight-line variation, where trigonometric or exponential variation would be expected. These anomalies remain to be explained.

The Reynolds Number.—Mr. Lieber properly raises the question of viscosity effects, represented by the Reynolds number. This would apply to oscillating models as well as static models. Wind-tunnel tests at the University of Washington, in Seattle, in which given section models were subjected to the widest possible range of variation of wind velocities, yielded practically identical lift and torque graphs at all wind velocities. The effect of the wide variation in the Reynolds numbers was found to be negligible. In the wind-tunnel tests at the Virginia Polytechnic Institute in Blacksburg, which supplied the graphs used by the writer, maximum wind velocites were used in order to minimize any deviation due to the Reynolds number.

Oscillating Model Tests.—Mr. Thomas comments on the advantages of oscillating models. It would certainly be a most happy consummation if someone could develop quick, inexpensive methods of making oscillating model tests that would be sufficiently accurate and that would yield all the required information. There remain, however, several problems yet to be solved before small-scale oscillating models will supply the answer. These unsolved problems include improved methods of pivoting and suspension for studying separately vertical, angular, and coupled oscillations; elimination of friction; elimination or complete measured control of structural damping; methods of carrying the tests down to the lower velocity ratios; methods of frictionless recording of the oscillations; and methods of measuring logarithmic increment or decrement (negative or positive damping). Even with the most elaborate and expensive equipment for oscillating model tests, both in the United States and abroad, results have been obtained that were misleading or incomplete, and significant relations have been missed that were revealed by the writer's pioneering theoretical analysis.

Even if simplified and perfected laboratory techniques for oscillating model tests become available, a comprehensive unifying scientific theory is still needed for correlation, control, guidance, and prediction. Aerodynamic theory was not scrapped when wind-tunnel testing was developed.

Coupled Oscillations.—In response to one of the questions asked by Mr. Dohn, the following notes on coupled oscillations may be helpful.

A section that has both vertical and angular stability over the entire wind velocity range, or over any substantial part of that range, may nevertheless be subject to instability in the form of combined vertical and angular oscillations, termed "coupled oscillations." In the case of airplane surfaces or combinations of airplane surfaces, otherwise aerodynamically stable, coupled oscillations may occur, producing the serious phenomenon of "flutter," which may attain destructive amplitude.

As far as is known, no coupled oscillations of actual bridges have ever been identified and recorded. Coupled oscillations have, however, been observed in wind-tunnel tests on bridge models, also in small demonstration models. Since coupled oscillations of bridge sections are a possible form of instability, the bridge engineer has to take such possibility into account.

Other questions asked by Mr. Dohn, although of real interest, are beyond the scope and province of the paper, and space limitations do not permit an adequate answer.

Effective Angle of Incidence.—The writer, calling attention for the first time to the difference between apparent angle of incidence and the true effective angle of incidence, stated that his formulas (Eqs. 11) covering this correction were written for a section model and would require appropriate modification for a bridge or a scale model of a bridge. Mr. Vincent is correct in noting that the additional moment due to camber would need to be considered in such amplification of the formulas.

The δ-Parabolas.—The writer's "δ-parabolas" are based on the assumption at Eqs. 19 that the static lift or torque graph is represented by a cubic equation. If the lift or torque graph differs from a cubic, the δ-amplitude curves given by the construction of Fig. 20 would not be parabolas; and, if the variation from a cubic is substantial, the simplified graphic solutions shown in Figs. 19 and 20 would need to be modified. The graphic short cuts and examples given were intended merely to be illustrative. The main objective was to demonstrate the writer's thesis that amplitudes are determined by the curvature of the static lift and torque graphs.

Contrary to the inference in Mr. Vincent's discussion, the δ-parabolas and the related graphic short cuts (Figs. 19 and 20) are not an integral part of the writer's theory and do not affect its basic accuracy. They were added merely as related items of interest and of suggested usefulness. They certainly do not affect the analysis for critical velocities and instability response in vertical, torsional, and coupled oscillations. The writer has concentrated on deriving and predicting the instability response curves because they are of the most vital significance in studying the potential instability of a structure. They are basic characteristic curves, unaffected by changes in stiffness, damping, or modes of oscillation. Amplitude response curves are of secondary value, and can be computed or plotted by various indicated methods from the basic instability response curves. Oscillating model tests yield incomplete information unless they give instability response curves. A hundred different amplitude response curves (for different modes and different damping values) are

required to give the information that is supplied by a single instability response curve.

The section models used in the static wind-tunnel tests for this paper were simple sections (thin plate and symmetrical **H**), without stringers, curbs, or flanges. Instead of inverting or reversing the model in the wind tunnel to cancel deviations from perfect symmetry, the raw wind-tunnel graphs were inverted and reversed, yielding the smooth, symmetrical, averaged curves passing through the origin. It is realized, of course, that unsymmetrical and modified sections will yield unsymmetrical and modified wind-tunnel curves. The numerical examples will be correspondingly modified, but the theory and its basic formulas are not affected.

The Negative Slope Theory.—Mr. Vincent states that the "'negative slope theory' * * * does not explain the successive restricted modes, each of which prevails over only a limited range of velocity." That is exactly what the writer's theory does explain (see Figs. 13 and 15 and other publications of the writer[75,76,77]). The writer's extension of the "negative slope theory" from its original limited application to narrow sections (such as ice coated transmission lines) to the more general and more complex case of wider sections (such as airfoils and bridge sections) introduced all the new aspects of the theory—notably: Torsional oscillations, phase difference, ranges of instability for "stable" sections, and successive ranges of limited instability (in addition to the range of catastrophic instability) for unstable sections. Since any given range of limited instability is expressed in terms of $\frac{V}{N\,b}$, an increase in wind velocity requires an increase in the frequency N for continued instability in the same range, and that is why successive restricted modes are evoked, each of which prevails over only the corresponding limited range of wind velocity. No other known theory explains and predicts all these observed phenomena and relationships, and no other known theory offers a quantitative basis for their evaluation.

Vortex Action.—Mr. Vincent makes the interesting suggestion that "vortex action" may contribute a superimposed effect. The writer has made an independent study, with original contributions,[73] covering "vortex action" in producing forced vibrations of cylindrical sections (sections of zero static lift). The essential characteristic feature is the appearance of the alternate vortex trail, with corresponding periodic alternations of lift force, even when the section is held stationary. To the writer's knowledge, tests have not revealed this characteristic feature of vortex action in static model tests of bridge sections. Alternating vortices observed in the case of an oscillating section model are a different phenomenon. They are explained by the oscillations, and are not the cause of the oscillations; the vortices then correspond to the known

[75] "Rigidity and Aerodynamic Stability of Suspension Bridges," by D. B. Steinman, *Transactions*, ASCE, Vol. 110, 1945, p. 548, Fig. 26.

[76] "Simple Model Tests Predict Aeroydnamic Characteristics of Bridges," by D. B. Steinman, *Civil Engineering*, January, 1947, Figs. 4, 5, 6, and 7 and February, 1947, Figs. 12 and 14.

[77] "Problems of Aerodynamic and Hydrodynamic Stability," by D. B. Steinman, *Proceedings*, 3d Hydraulics Conference, *Bulletin No. 31*, State Univ. of Iowa Studies in Eng., Iowa City, Iowa, 1947, Fig. 11.

alternations of lift force on the oscillating section, and the period of the vortex discharge is determined by the period of the oscillations. Whether there is, in addition, an independent vortex action (of independently determined period) superimposed upon the known aerodynamic forces, requires further research. The present indications are that any such superimposed independent vortex action is imperceptible or negligible in effect. It may contribute a refinement in the theory, but it certainly does not offer an independent theory explaining the known phenomena of bridge oscillations.

Limitations of the Conventional Flutter Analysis.—Mr. Vincent advocates the use of the Theodorsen flutter analysis for the preliminary examination of proposed bridge designs. Aside from the inaccuracies and inconsistencies involved in applying the conventional flutter analysis to non-airfoil sections, such procedure would completely miss the investigation of potential vertical and torsional oscillations. All it would give would be a value (of questionable accuracy) for critical velocity for coupled oscillations, with no information on the intensity of potential amplification (negative damping) and potential amplitude response for vertical, torsional, and coupled oscillations. It would also give no information on the low velocity ranges of limited instability. After all, the challenging problem is that of vertical and torsional oscillations, whereas coupled oscillations, in the case of bridges, are still hypothetical and unrecorded.

Influence of Terrain.—The writer believes that much valuable time and effort have been wasted in seeking some mysterious influence of the terrain in the investigations of aerodynamic effects. All that need to be known in reference to any bridge exposure are the potential velocity, direction, and inclination of the wind. Wind gusts are only one of the many possible causes of initiating small oscillations, but wind gusts will not explain the amplification that constitutes instability. It is the steady component of the wind that produces continuing and amplified oscillations.

The writer has stressed the significance of wind inclination in his prior published writings, and he is in full agreement with the last paragraph of Mr. Vincent's discussion.

Comparison with Oscillating Model Tests.—Mr. Farquharson challenges the validity of the writer's analysis by emphasizing quantitative deviations between selected results from various oscillating model tests and the values plotted in the writer's numerical examples. The comparison is not valid for a number of reasons. In the first place, the sections used in the oscillating model tests were not the same as those used in the writer's numerical examples; there were material differences in both form and proportions. The cross section Mr. Farquharson has added in Fig. 28 is incorrect for the graph above it and is therefore misleading. He states, elsewhere in his discussion, that these differences in the cross section are of "great importance." In addition, the writer's numerical examples used assumed or approximate data (including roughly approximate preliminary values of the slopes s_1, s_2, s_3, and s_4), and were intended merely for qualitative and illustrative use and not as the basis for any definitive critical test of validity. Moreover, different oscillating model tests made by Mr. Farquharson at different times and under different conditions

have yielded varying results, and tests made by Louis G. Dunn and others at the California Institute of Technology in Pasadena have yielded still different results. By selecting data from such varying range, almost any kind of case can be made. By the same reasoning, anyone who was so inclined could easily challenge the validity of Mr. Farquharson's oscillating model tests.

In the California tests cited by Mr. Farquharson, using a section of the same form as the writer's and differing only in the $\frac{d}{b}$-ratio, much closer agreement with the writer's computed values was obtained. The range of the observed oscillations indicates maximum instability approximately at $\frac{V}{N\,b} = 1.75$ compared with the writer's value (Fig. 13) of $\frac{V}{N\,b} = 1.60$. In the actual graph, this displacement would be relatively small. The small difference in the $\frac{d}{b}$-ratio is sufficient and in the right direction to explain the disparity.

Had Mr. Farquharson presented an instability response graph (as distinguished from an amplitude response graph) it would be directly comparable with the writer's instability graphs (Figs. 13 and 15). Such direct comparison would provide a direct check of the basic critical velocities (at zero damping) as computed by the writer. Instead, Mr. Farquharson gives critical velocities (for the principal range only) at various high damping values. As a result, an important error is introduced since all of his values of structural damping are too high. They need to be corrected by subtracting the atmospheric damping in still air. The writer's formulas and his graphs include automatic correction for atmospheric damping; but Mr. Farquharson's damping values, obtained by oscillating the model in still air, include atmospheric damping and high friction damping as well as structural damping. All his horizontal lines need to be lowered an undetermined amount, and this will modify the comparisons he has made and the conclusions he has drawn.

Mr. Farquharson declares that dynamic model tests have indicated values for the logarithmic increment well above the maximum of $\delta_1 = 0.0152$ shown in Fig. 13, and cites observed vertical motion at $\delta_s = 0.021$ in the California tests and much higher values in his own tests. There are two errors in this comparison. In the first place, he bases his conclusion on his values of δ_s, and all the cited values of δ_s need to be corrected downward an unknown amount, as explained herein. In the second place, the ordinates of any instability graph depend upon the arbitrarily assumed value of the width factor b^2/w (representing the nondimensional density-mass factor). All the ordinates may be increased or diminished in any desired ratio by increasing or diminishing the assumed value of b^2/w in the same ratio. Without giving the values of b^2/w he used, in comparison with the value assumed by the writer in the numerical example, Mr. Farquharson's comparison is invalidated.

The form of the amplitude response graphs obtained by Mr. Farquharson (shown in Fig. 29) constitutes a truly remarkable check of the writer's theory as represented in the amplitude response graph derived in Fig. 22. No other known theory will yield these results.

Lower Instability Ranges.—Mr. Farquharson agrees that his oscillating model, by its limitations, is incapable of revealing or disproving the lower instability ranges given by the writer's theory and shown in Fig. 13. Consequently, it cannot be used to establish the existence or nonexistence of such lower instability ranges. He admits that the California tests showed the second or intermediate range, although he could not get that range on his model because of its excessive structural damping (0.0068 and higher), including friction damping.

Mr. Farquharson then adds that structural damping values as low as 0.0025 could never be expected on any bridge yet built. On the contrary, the actual structural damping value on the original Tacoma Bridge was as low as 0.002, or lower. The girders and roadway contributed only 1.5% of the total stiffness; consequently, their structural damping value is multiplied by the low factor of 0.015. The remainder of the stiffness was given by the gravitational resistance of the cables, and such nonstress contribution adds nothing to the structural damping. The observed persistent oscillations of the bridge in various modes at low wind velocities are further proof of the low damping value.

With the principal instability range and the second instability range conceded, all considerations of mathematical and physical continuity point to the logic of a series of diminishing instability ranges converging toward the origin. The trigonometric integrals that are necessarily involved yield such a tapering sinusoidal graph. This is one place where the writer's theory reveals relationships that are beyond the limitations of oscillating model tests.

Contrary to Mr. Farquharson's surmise, the writer did not derive the lower instability ranges from the plotted diagram in the Carmody report (the official report of the Tacoma Bridge failure). The originally published diagram in that report[60] is unscientific. It confuses and obscures the true picture by showing observed oscillations plotted on velocities V, whereas they should be plotted on velocity ratios $\frac{V}{N\,b}$. This would make the mode of oscillation immaterial, thereby yielding cumulative massed data from all the modes. The writer, following this principle, obtained the instability ranges by plotting all the recorded observations from the official log, after reducing each observation to its value of $\frac{V}{N\,b}$. When the observations were so plotted, the successive critical ranges were clearly revealed. Subsequently, Mr. Farquharson reported a calibration correction for the anemometer used in the observations,[61] which modifies the critical ranges previously revealed but does not nullify them. Instability ranges at low $\frac{V}{N\,b}$ remain confirmed by the recorded observations of the higher modes at low wind velocities within the calibration range.

Subharmonic Oscillations.—The writer used the appellation "subharmonic," in quotes and in parentheses, merely to facilitate identification by those who had applied that designation. The writer holds no brief for the correctness of that term. In fact, he had long ago discarded the implied concept of forced vibrations due to vortex action as having any useful validity for the bridge application.

Incidentally, a bridge is a linear system with respect to oscillations, and (contrary to Mr. Farquharson's conclusion) it is impossible for a linear system to be excited to amplified oscillations at ½ (or any other submultiple) of the frequency of the disturbance.[65] This basic fact would seem to eliminate vortex action as the explanation of the phenomena of bridge oscillations.

Torsional Instability Graphs.—Mr. Farquharson's comment on the torsional instability graphs in Fig. 15 is hairsplitting. Even if he is correct in noting that the two parallel lines are only approximately parallel and would meet somewhere outside the figure, the agreement of the two curves in Fig. 15 still constitutes a remarkable check of the writer's theory.

That Mr. Dunn's experimental graph in Fig. 15 shows too high a value for the critical velocity is established.[22] The writer's conclusion was not based on the laboratory data (which would not have revealed the error), but was derived from internal evidence in Mr. Dunn's instability graphs as published in the official report.[21] For different values of the spring constant and of the density-mass ratio, the graphs showed different critical velocity ratios, ranging from 3.3 to 5.1, whereas the values should have been identical. The only consistent explanation was that, through some unknown experimental error, the plotted zero line had been displaced upward from the true zero line at which the different graphs (having different slopes) would meet at a common point (allowing for minor experimental errors). Since the plotted zero line was determined by subtractive elimination of the structural damping (lowering the zero line), the only reasonable conclusion was that the subtractive correction had been too small, and that some contribution to the total damping (possibly the nonlinear friction damping) had been missed or overlooked by the investigators. The graph needed to be raised relative to the zero line, thereby reducing the indicated value of the critical velocity ratio. The shift derived by the writer reduces the critical ratio $\frac{V}{N\,b}$ from Mr. Dunn's value of 3.3 (ranging to 5.1 in his other tests) to a lower corrected value of 2.5. Mr. Farquharson agrees that this shift is in the right direction, but indicates that it does not go far enough, since he now cites values as low as 1.9 at zero damping, 2.01 at low damping, and 2.5 at $\delta_s = 0.03$ (which would be a lower figure after subtracting atmospheric damping). Earlier reported tests gave values of 2.6 to 2.8 for the critical velocity ratio for the Tacoma section. The variation in the reported results of oscillating model tests by different experimenters at different times, with their respective values of critical velocity ratio ranging from 5.1 down to 1.9, is far greater than the variation of the writer's computed value of 2.5 from the values now cited by Mr. Farquharson. The difference in section ratios is in the right direction to explain the variation.

Mr. Farquharson does not challenge the shape of the writer's torsional instability graph shown in Fig. 15; nor does he offer a corresponding instability graph, obtained experimentally, for direct comparison.

A Scientific Check of the Theory.—The writer would welcome an open-minded, cooperative, scientific check of his theory by oscillating model tests.

[22] "Rigidity and Aerodynamic Stability of Suspension Bridges," by D. B. Steinman, *Transactions*, ASCE, Vol. 110, 1945, p. 569.

Only by tests on identical sections, under identical conditions, can a scientifically valid comparison be made. Such tests would either confirm the validity of the writer's theory, or indicate the direction of study for any correction or further refinement. Mr. Farquharson agrees, in the opening sentence of his discussion, that such consummation "would be fortunate indeed."

Testing of Theories.—There is a habit of blindly accepting a mathematical analysis, without checking the derivation and investigating its limitations, merely because the final formula appears to check certain experiments; and there is a corresponding habit of blindly rejecting a mathematical analysis, however scientific and rational, merely because the final formula does not appear to check certain experiments. Both of these procedures are unscientific. The accepted theory may contain inconsistencies, anomalies, and limitations that are not revealed by the experimental tests to which it has been applied. The other theory may have been tested unscientifically or inaccurately, or a single link in the theory may require modification, and premature rejection may mean the loss of a scientific contribution to progress.

It is true that the Theodorsen flutter theory has been checked by experiments on streamline airfoils, but that does not necessarily prove that the theory and its derivation are perfect. Certain anomalies and inconsistencies remain unexplained. Moreover, that theory cannot be applied to nonairfoil sections—not even to thin flat plates—without violating the underlying assumptions implicit in the analysis.

The writer's theory has not been tested by application to streamline airfoil sections. It may check such tests as well as the Theodorsen theory, even though the phase-correction coefficients are not identical in the two theories. In the airfoil application, the velocity V is relatively high and the width b is relatively narrow, so that the total phase difference across the section is relatively small. On account of the smallness of the total phase difference across the section, any inconsistencies or imperfections in the Theodorsen theory may not be revealed. Moreover, at high values of $\frac{V}{N\,b}$, the two theories are in virtual agreement and, at the limit, the two theories are identical.

The real test of the writer's theory is in the application to bridge sections, for which the theory was written and to which the Theodorsen theory is admittedly inapplicable. This is a far more difficult and complex problem, beyond the resources of classical aerodynamics. The writer's theory is the only analysis that explains all the known phenomena of bridge oscillations, the only analysis that shows remarkable qualitative confirmation, and the only analysis that offers a quantitative solution for all phases of the problem. The scientific attitude demands fair consideration and testing before condemnation.

Critical Review.—The writer has not claimed finality of perfection for his theory, nor has he asked blind acceptance. He has submitted it to the profession for critical analysis, constructive review, and scientific checking. The fact that it contains new ideas and departs from conventional theories that have become sacrosanct is not a rational reason for prejudiced attack or blind rejection. That is not the way to make progress.

As stated in the paper, Eqs. 2 are the pivotal formulas of the analysis pre-

sented. They are the equations giving the air forces acting on any oscillating section. They are new, and represent a contribution of basic importance. To the writer's knowledge, the problem of writing a general formula for the air forces acting on oscillating sections has never been solved before. Derived with absolute mathematical rigor, the basic validity of Eqs. 2 cannot be successfully challenged. No assumptions, approximations, or short cuts are involved in their derivation. If the writer had ended his paper at the establishment of Eqs. 2, that contribution alone would have justified publication as a scientific advance and as a basis for further progress. In order to serve maximum usefulness, however, the writer decided to give the remainder of his analysis.

The pivotal formulas (Eqs. 2), written with complete generality, contain four coefficients, C_1, C_2, C_3, and C_4, the phase-correction factors (approaching the value of unity at the limit). These four coefficients are given rationally and scientifically by Eqs. 5. The basic validity of Eqs. 5 cannot be challenged successfully. They involve the use of the phase factor $e^{-i\varphi}$. That use of the phase factor $e^{-i\varphi}$ is not an arbitrary assumption, as asserted by Mr. Bleich. It is rigorously established in vibration mechanics.

All the essential formulas that follow in the paper are of unquestioned rigor and validity. They are so written as to remain unchanged if any desired modification or substitution is made for the F-values and G-values given by Eqs. 5.

The writer has also extended and applied the formulas to unsymmetrical sections (with laterally eccentric center of gravity or stiffness axis, or both), with strikingly interesting results checking the known behavior of oscillating models, previously unexplained. Space limitation has prevented the inclusion of this material.

The value of the writer's contribution, subject only to possible future refinement, is trustfully submitted to the judgment of the profession.

Acknowledgments.—The writer wishes to express his heartfelt gratitude to all who gave him encouragement and cooperation at a time when he felt the loneliness of his task. In particular he wishes to record his indebtedness to Messrs. Maher, Perry, Peterson, Bakhmeteff, and Tea for personal services, encouragement, and valued cooperation in making required tests. Hunter Rouse and Joseph W. Barker, Members, ASCE, and Harry D. Belock, respectively, made available the facilities of: The Iowa Institute of Hydraulic Research in Iowa City, Iowa; the Virginia Polytechnic Institute through the Research Corporation; and the Reeves Instrument Corporation. Messrs. Allan, Klemin, Thomas, Lieber, Slavin, Richardson, Ghaswala, Dohn, and others named herein are thanked for their generous discussions.

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